# BRAITHWAITE'S RETROSPECT.

VOL. LXV. JANUARY—JUNE, 1872.

THE

# RETROSPECT OF MEDICINE:

BEING

## A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D., LOND.

ASSISTANT-SURGEON TO THE LEEDS HOSPITAL FOR WOMEN AND CHILDREN.

VOL. LXV. JANUARY—JUNE.

1872.

LONDON:

SIMPKIN, MARSHALL, AND CO.

EDINBURGH: OLIVER AND BOYD. DUBLIN: HODGES, FOSTER, AND CO. AND FANNIN AND CO.

MDCCCLXXII.

*Letters for the Editors, to be addressed*

Dr. Wm. Braithwaite, Clarendon House, Clarendon Road, Leeds; *or*

Dr. James Braithwaite, Clarendon Road, Leeds.

*Parcels of Books, &c., to*

Messrs. Simpkin, Marshall, & Co., London.



PRINTED FOR D. I. ROEBUCK,
AT THE OFFICE OF THE YORKSHIRE POST, ALBION STREET, LEEDS.

# CONTENTS OF VOL. LXV.

---

## SYNOPSIS.

---

## PRACTICAL MEDICINE.

### DISEASES AFFECTING THE SYSTEM GENERALLY.

## SYSTEM GENERALLY, (CONTINUED.)



## DISEASES OF THE NERVOUS SYSTEM.



## DISEASES OF THE ORGANS OF CIRCULATION.



## ORGANS OF RESPIRATION.



## ORGANS OF DIGESTION.



## URINARY ORGANS.

# SURGERY.

## AFFECTIONS OF THE BONES AND JOINTS, FRACTURES, DISLOCATIONS, &c.



## ORGANS OF CIRCULATION.



## ALIMENTARY CANAL.

## ORGANS OF URINE AND GENERATION.



## AFFECTIONS OF THE EYE AND EAR.



## AFFECTIONS OF THE SKIN.



## SYPHILITIC AFFECTIONS.

## MIDWIFERY,

### AND THE DISEASES OF WOMEN AND CHILDREN.



---

## INDEX.

---

## GENERAL INDEX, Volumes LIX.—LXIV.

# SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

---

## AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—The most recent views of the nature of cancer are, that it is not a constitutional disease at all at first, but purely local; and that by subsequent absorption of its elements the system becomes affected. It may be said to be hereditary, in the same way as a tendency to any other affection (as warts) is hereditary. The use of caustics after removal of the mass of the tumour by the knife is coming more and more into favour. Chloride of zinc is especially suitable. (Mr. H. Arnott, p. 91.)

*Condurango in Cancer.*—Condurango is a climbing plant growing on the western slope of the Andes in Ecuador. It is said to be a remedy of value in the cure of cancer. The powdered bark is the part used, and is 'the rage' in New York at the present time. Its action, however, must be more definitely ascertained before we should advise any one to trust to it. (Dr. Bliss, p. 376.)

CHOLERA.—*Nitrite of Amyl.*—Nitrite of amyl causes great dilatation of both the systemic and pulmonary capillaries. The blood flows rapidly through the capillaries into the venous system, and the pressure on the arteries becomes correspondingly diminished. As, from the observations of Dr. George Johnson, it appears certain that the collapse of cholera is owing to contraction of the pulmonary capillaries and arrest of the circulation commencing there, it seems not improbable that this peculiarity of the action of nitrite of amyl may be the very thing wanted in cases of cholera collapse. It would have to be given by the stomach, or subcutaneously. It has not yet been tried in actual practice. (Dr. J. L. Brunton, p. 88.)

*Subcutaneous Injection of Morphia.*—Dr. Patterson, of Constantinople, reports that in the late epidemic of cholera at that city, finding all other treatment unsatisfactory, he determined to try the subcutaneous injection of morphia. In the

first case a quarter of a grain of the acetate caused relief to the cramps and vomiting in a quarter of an hour, and the skin became gradually warm and moist, and the pulse returned. In ordinary cases he found one or two injections sufficed, in a few three were given, and only once four. He does not maintain that the treatment is a specific against cholera, but that its action is more speedy, certain, and effectual than any other tried by him. Out of 32 cases in which the treatment had a fair chance, there were only ten deaths. (Dr. J. Patterson, p. 87.)

FEVER.—In severe cases of fever, what our efforts should be principally directed to avert is stagnation of the blood in the small vessels, and cessation of the capillary circulation over a considerable part of the body. The principal cause of this is failure of the heart's action. During the critical period it may be necessary to excite the organ to increased action by the administration of stimulants, and it is very important to watch carefully for any indications of approaching failure. (Dr. L. S. Beale, p. 36.)

*Incubation of Typhus, Relapsing, and Enteric Fevers.*—The period of incubation of typhus is about twelve days, of relapsing about nine days, and of enteric (typhoid) fever about two weeks. These periods are, however, extremely variable, and must be only taken as averages. (Dr. Murchison, p. 12.)

*Extreme Heat in Fevers.*—As bearing upon the important question of restraining extreme heat of the body in disease, we must remember that, whereas the nitrogen-containing tissues are hardly consumed at all in circumstances of health, the febrile state involves a large destruction of them, and this at a time when, from the state of appetite and primary digestion, scarcely any supplies of nitrogenous food can be taken into the system. Every additional degree of fever heat implies so much additional destruction of the most important organs of the body, including the heart and the nerve centres. Excessive heat does not mean plenty of strength, but exactly the opposite; it is an absolute proof that the reserve forces of the body are exceedingly low, and are being constantly and rapidly reduced. (Editor of Lancet, Feb. 3, p. 153.)

HYPERPYREXIA.—*Treated by Withdrawal of Heat.*—It is a fact which no theory can alter, that if the temperature of the body rise up to, or above 107°, death is imminent; partly from the effect of the excessive heat on the vitality of organs, and partly from the excessive tissue-change which must be going on to produce this heat. Excessive and very rapid

rise of temperature is seen more frequently in acute rheumatism than in any other disease. Dr. Wilson Fox relates a case in which, when the temperature was 107° in the rectum, the patient was placed in a bath at 96°; the heat, however, continuing to rise to 110° in the rectum, iced water was used and ice placed down the spine. In half-an-hour the temperature was reduced to 103·6°, and in another half-hour to 99·5°. During this time six ounces of brandy were given, and subsequently eighteen ounces a day for several days. (Dr. L. S. Beale, p. 26.)

The statistics published in Germany, comprising more than a thousand cases of typhoid fever, treated in various hospitals, show that the mortality in that disease has been reduced by more than one half by the cooling treatment. This consists in not allowing the temperature to rise above a certain standard. If it does, it is at once lowered by baths or other means. This method of treatment is therefore not directed against the disease itself, but simply against a symptom, upon which a series of other symptoms depend. The patient is not plunged in a cold bath, but in one ten degrees below the heat of his body. It is only in certain cases that the use of absolutely cold water or ice is required. As many as ten or twelve baths in the twenty-four hours are sometimes required, in order to keep down the excessive fever heat. (Editor of British Medical Journal, p. 53.)

There can be no doubt that some cases prove fatal simply from the excessive temperature of the body consuming the tissues so rapidly. No case can long go on favourably with a temperature of say 106°, and as it is proved by a considerable amount of experience, especially in Germany, that an excessive temperature may, without any danger, be lowered artificially, this plan of treatment is imperatively called for in cases where an excessive temperature exists. Withdrawal of heat must, however, be practised with judgment, and not too rapidly, as the temperature will then swing back to and even beyond its former place. The rule ought to be, that the withdrawal of heat should be the slowest possible for the due attainment of our end. The patient should be placed in a bath of 100°, and the temperature of this gradually lowered, so that no shock is given to the system. On the removal from the bath quinine in doses of from five to twenty grains, according to circumstances, as advocated by Binz, will arrest the rapid reproduction of heat. This mode of treatment requires the incessant use of the thermometer, and the personal supervision of the medical attendant. A very interesting and successful illustrative case is related: it was one of cerebral

rheumatism, which had supervened upon an ordinary attack of rheumatic fever, without heart affection. The temperature was 105·8°. A bath not being at hand, ice was put in five wide-necked bottles and placed in bed, close to the body, and ice also applied to the head. The cold was thus applied partially, and the same effect produced as by a less degree of cold applied to the whole surface of the body. (Dr. T. C. Allbutt, p. 61.)

*Dr. Roberts's Cooling Pad.*—The use of cold baths, and ice, for the reduction of temperature, in cases of hyperpyrexia, is attended with a good deal of difficulty in private practice. The author recommends the use of a pad consisting of circular coils of india-rubber tubing, cemented to a canvas backing. The pad is laid on the patient's body, and a constant stream of water, of any desired temperature, passed through it. It is found that the effluent water is raised from twenty to thirty degrees, and the temperature of the patient can be lowered two to four degrees. This is not effected so rapidly, nor can the lowering be carried on to the same extent as when a bath is used, but quite sufficient for any but exceptional cases. (Dr. W. Roberts, p. 57.)

RHEUMATISM.—*The Synthesis of.*—The author relates some experiments which were made on two persons, in which all the symptoms of acute rheumatism were produced by the internal administration of lactic acid. The acute articular rheumatism came on when the acid was taken, and ceased when it was discontinued. Coinciding with the development of the articular affection was the appearance of perspiration, in the severe attacks copious and acid. (Dr. B. W. Foster, p. 67.)

SCARLATINA AND SMALL-POX.—*Copaiba.*—Copaiba, administered internally, has the effect in cases of scarlatina and small-pox, of causing a free flow of urine, and rapid improvement in the general symptoms. It seems as if it had some direct action upon the virus of the disease, altering and destroying its character. (Dr. Rowand, Quebec, p. 84.)

SMALL-POX.—*The Antiseptic Treatment of.*—The apices of the pustules should be touched with pure carbolic acid, the odour of which may be effectually disguised by a little oil of wild thyme. A solution of one part of carbolic acid in three of olive oil should then be applied over the individual pustules night and morning. The pustules become white, and dry up when treated in this manner. The general surface of the body may be washed with any soluble antiseptic,

such as coal-tar soap. Sulphite of sodium should be given internally in scruple doses every four hours. By this means the patient is disinfected from the outset of his malady. (Dr. A. E. Sansom, p. 77.)

The secondary fever of confluent and semi-confluent cases of small-pox is due to the presence in the body of products of decomposition, which commence to be formed as soon as the lymphy contents of the hitherto vesicles become purulent. The dermatitis which springs into existence at this period is a necessary consequence of the irritation of the now numerous sub-epidermic abscesses. Sulphurous and carbolic acids, are agents capable of checking the decomposition of the pus, and so obviating the consequent septicæmia. The sulpho-carbolate of sodium should be given internally in doses of from seven grains occasionally, to sixty grains every third hour. Sulphurous acid much diluted with water should be used to wash the body with, and sulphur burnt in the room, so as to impregnate the air with sulphurous acid. Diluted sulphurous acid may also be given as the usual drink. (Dr. A. W. Foot, p. 79.)

In an outbreak of small-pox which occurred on board a U.S. vessel at Yokohama, Japan, the free internal administration of hyposulphites (in the absence of sulphites) proved of the most marked use in checking the disease. The good effects of the salt were generally manifest after the first dose; the patient losing the heat and dryness of the skin, and expressing a feeling of relief. There was produced not only a subsidence of the fever, but a tardy or incomplete development of the eruption, and that in an epidemic of unusual severity. (Dr. W. A. Corwin, p. 82.)

Mr. Stephens, of Plymouth, who has had charge of two small-pox hospitals at that town, recommends strongly the local application to the pustules of a mixture of olive oil and carbolic acid. By this means the patients are themselves disinfected, and rendered innocuous to the community at large. (Mr. J. N. Stephens, p. 84.)

*Prevention of Pitting in Small-Pox.*—The reason that pitting occurs only or chiefly on the face is, that it is exposed to the air, and the scales become hard and dry, whilst the body is in a state of comparative humidity. In a most severe case of small-pox, with extraordinary heat of the face, in the hope of saving the eyes, poultices were applied over them. The patient recovered, but with deep and permanent pitting, except where the poultices had been applied. Since this case

occurred to him, Dr. Stokes has adopted as a routine practice the application of light poultices over the entire face, or of a mask of lint steeped in glycerine and water, and covered with a corresponding mask of oil silk; and he has found that, with but one exception, the pitting was effectually prevented. (Dr. W. Stokes, p. 73.)

*Warm Baths in Small-pox.*—The relief which is given in a severe case of small-pox by a warm bath is very great. It soothes and softens the inflamed and parched skin, relieving pain and procuring sleep. This treatment is called for towards the later rather than the earlier stages of the disease, when there are black fœtid scabs. In such a case all pain will be relieved, and all fœtor immediately and completely destroyed. (Dr. W. Stokes, p. 76.)

TYPHOID FEVER.—*Use of the Thermometer in.*—What use is the thermometer in the diagnosis of the typhoid fever? During the first four days the temperature rises one degree each day, starting at 98$\frac{3}{5}$ the morning of the first day, but the evening temperature is always two degrees above that of the morning of the same day. The disease is therefore not typhoid fever, if, (1) on the second, third, or fourth evening the temperature approximates even to the normal (98·6° Fahr.); (2) if, during the first two days, the temperature rises to 104° F.; (3) if before the fourth and sixth days the evening temperature of a person under middle age does not reach 103°; (4) if the temperature on two of the first three evenings is the same; or (5) if it is the same on the second and third morning. (Dr. P. W. Latham, p. 15.)

*The Bowel Lesion of Typhoid Fever.*—The generally entertained opinion that the bowel lesion is the result of nature's efforts to eliminate, is entirely erroneous. Were thie this true bowel lesion would relieve rather than aggravate the constitutional symptoms. The inflammation of the agminated and solitary glands bears exactly the same relation to the fever that the sore throat of scarlet fever does to that disease, that is, it is the direct effect of it. No doubt the sloughs and discharges from the ulcerated glands carry the poison of typhoid fever and are capable of conveying the disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina are capable of transmitting their peculiar poison. (Dr. Maclagan, p. 41.)

*Lacto-phosphate of Lime in Typhoid Fever.*—Lacto-phosphate of lime is at once an aliment and an article of food, and a medicament of the highest value. It excites the appetite and facilitates digestion. In cases of acute disease, especially

typhoid fever and forms of inflammation, it is most valuable, acting to some extent in the same way as alcohol. The first effect is that the pulse becomes less frequent and the temperature lower. It is especially useful during recovery, being at once the chemical agent of digestion and the natural excitant of nutrition. (M. Blacke, Practitioner, Feb. p. 65.)

---

## AFFECTIONS OF THE NERVOUS SYSTEM.

Alcoholic Paraplegia.—Chronic alcoholism affects the whole nervous system, but the spinal cord is the part most prone to suffer. This is not uncommon in females. The condition of the patient is this: She lies in bed or on a couch complaining of severe pains in all the limbs, more especially in the lower ones, which are much wasted, or of a sensation like electric shocks running through them, together with numbness and considerable anæsthesia, and at the same time only slight power of movement, or total inability to stand. There is generally enlargement of the liver, with sickness and all the usual signs of chronic alcoholism. The only treatment required is resolutely to break off the stimulants. There need be no fear of inducing delirium tremens, however much has been taken, and however suddenly it is discontinued. (Dr. S. Wilks, p. 106.)

Nervous or Sick-Headache.—In those forms of sick-headache which are preceded by disturbed vision, or other signs recognisable by the patient as preceding an attack, the patient should lie down with the head as low as possible, and if the glimmering be on the right or left of the field of vision he should lie on the opposite side. He should at the same time take some powerful diffusible stimulant. By this means the defective supply of blood to some portion of brain, which is the real disease, is counteracted. There is always a loss of tone about the cerebro-spinal system in cases of this kind, and of course all measures calculated to improve the general health should be adopted. If the attack is followed or preceded by great mental depression nothing acts like half a drachm or a drachm of the ammoniated tincture of valerian. A remedy which is often given with great advantage during a severe attack is bromide of potassium in doses of 5, 10, to 30 grains, combined with 30 or 40 minims of sal volatile. If the attacks have been very frequent, or if there be any scrofulous tendency, the iodide of iron may be given in the following form: ℞. Ferri et ammon. cit., gr. v.; potassii iodidi gr. ij.; aquæ ℥j.; and, according to circum-

stances, 15 to 20 minims of tincture of henbane, or 20 or 30 minims of aromatic spirit of ammonia may be added. If the stomach is irritable this may be given in the effervescing form. In other cases citrate of iron with ammonia and strychnine may be given with great success. (Dr. P. W. Latham, p. 102.)

Neuralgia.—*Galvanism.*—In order to cure neuralgia by galvanism we should use the continuous current, by means of small wet sponges attached to small conical conductors. The constant galvanic current has a truly marvellous effect over pain, whereas the interrupted current is of little or no service. The battery used should be Weiss's, sometimes known as Foveaux's. Eight cells of this splendid battery suffice, with very small sponges (about as large as would fill the end of a thimble). They should be applied to the painful part, an inch or two apart, and moved about, without being actually removed from the skin, for about two minutes. After resting a minute they should then be applied again for two minutes. Three very interesting cases illustrative of this plan of treatment are given. (Mr. J. Stead, p. 112.)

Perivascular System of the Brain. — The vessels of the brain substance are themselves contained in a larger, rather loose, structureless membrane, completely surrounding the vessel. This membranous investment is easily detected with the naked eye in the fresh vessel; in some places a large space lying between them, in others the membrane lies so close to the vessel that it is with difficulty detected. Everywhere these membranous canals exist around the vessels, and they are quite sharply defined externally, as is proved by injecting them with wax. The use of these canals is probably to act as a reservoir for the fluid which exudes from the vessels, in order to permit of the rapid nutritive changes which must be necessary in such an organ as the brain. This perivascular system was first described by Robin in 1855. (Mr. W. W. Wagstaffe, p. 98.)

Restoration of the Functions of a Nerve after Excision of a Portion.—The author reports a case in which he excised a small neuroma of the ulnar nerve, along with half-an-inch of the nerve in its entire thickness, bringing the divided ends together with carbolized silk ligature, and closing the wound, which was treated on strictly antiseptic principles. The functions of the nerve had been in abeyance for nine years, and yet in fifteen days sensation had completely returned everywhere, except at the extreme tips of the two fingers. (Mr. T. R. Jessop, p. 110.)

## AFFECTIONS OF THE CIRCULATORY SYSTEM.

HEART AFFECTIONS.—*Digitalis.*—Digitalis is not merely a temporary remedy in cases of insufficient power of the muscular walls of the heart. It may be given continuously and uninterruptedly for years with excellent effect. With this, however, we should not forget the great necessity and advantage of rest, and improvement of the nutrition of the heart itself by iron, cod-liver oil, arsenic, and other remedies which increase blood formation. (Dr. J. M. Fothergill, p. 118.)

POPLITEAL ANEURISM. — *Compression.* — When the artery is ligatured, consolidation of the aneurism usually occurs within four hours. This is owing to the complete arrest of circulation through it. In applying pressure for the cure of the disease, we have to imitate this complete arrest. The weight used should be 16½ or 17 lbs., and the apex rather conical, so as to penetrate deeply. It should first be placed on the artery at the groin, and retained there about two hours, and then be removed to the superficial femoral in the middle of the thigh. It is a good plan to administer a grain of morphia to deaden the pain which will be caused. By alternating the position of the weight we gain this great advantage, that the task of establishing a collateral circulation is transferred from one set of branches of the femoral to another, and time is not given to either to do this, so that a collateral circulation is not established for a much longer period than if the weight is maintained in one place. The edge of the pubic ramus is the place where the artery can be compressed most easily and with least pain to the patient. Pressure applied diagonally to the bone is better than that applied perpendicularly to it, a less weight by two or three pounds being required. The groin ought to be well shaven, and powdered very thickly. It is of the greatest assistance to have the weight cool, and the upper part of it should be deeply hollowed for the reception of a frigorific mixture. (Mr. G. E. Walker, p. 205.)

*Double Tourniquet, with Shifting Pressure, for the Treatment of Popliteal Aneurism.*—It is extremely difficult, with the single tourniquets usually employed, to maintain the instrument in a fixed and immoveable position upon the thigh, without screwing down the compressing pad so tightly upon the artery as completely to obliterate its tube, in some cases to produce agonising pain. By the use of a double tourniquet we get over these difficulties completely, and can regulate the flow of blood through the artery to a nicety, relaxing one tourniquet pad, and screwing down the other, as may be

necessary to relieve the pain produced by the pressure. It defeats our object if we completely arrest the blood current, as a slow and feeble current is necessary for the deposition and lamination of fibrine in the cavity. (Mr. F. E. Bulley, p. 211.)

---

## AFFECTIONS OF THE RESPIRATORY SYSTEM.

Asthma.—Asthma should, with a view to its successful treatment be viewed as a neurosis of the pneumogastric nerve, of which sometimes the cause is disturbance of healthy function at the brain end, and sometimes at the gastric or hepatic. Thus bismuth and hydrocyanic acid are of great value when the neurosis is of gastric origin. Carlsbad salt, nitric acid, and at times small doses of mercury are all unmistakeably curative when the hepatic system requires relief. Other remedies such as ipecacuanha, belladonna, and nux vomica, are of use in appropriate forms of pneumo gastric disturbance; whilst iodide of potassium, sulphur, and arsenic are the remedies indicated if there is a gouty or rheumatic diathesis at the root of the malady. (Dr. J. C. Thorowgood, p. 134.)

Asthma, Angina Pectoris, and Gastralgia.—These three affections are intimately allied being neuroses of different portions of the pneumogastric nerve, of central origin. These neuroses are in great measure owing to inherited peculiarities of the central nervous system. There is one remedy which is of supreme efficacy in all these affections, namely, arsenic. In doses of five minims of Fowler's solution it gives rapid relief in many otherwise obstinate cases. It is particularly useful in that form of gastralgia which accompanies asthma. There are a certain number of persons who cannot tolerate arsenic, and the subcutaneous injection of the $\frac{1}{120}$th to the $\frac{1}{60}$th of a grain of strychnine should in that case be tried. If given by the mouth, the proper dose is from $\frac{1}{40}$th to $\frac{1}{24}$th of a grain three times a day. (Dr. F. E. Austie, p. 129.)

Hemoptysis.—*Ergotin.*—The subcutaneous injection of five grains of ergotin is one the most rapid and effectual means we have of arresting profuse hemoptysis. It produces contraction of the muscular coat of the arteries, and so diminishes the blood pressure. This is no mere theory but has been established by direct experiment. (Dr. Ritchie, p. 137.)

Hooping Cough.—To a child five years of age give the thirty-second part of a grain of morphia, with three grains of bromide of potassium, in solution, every two hours; letting

the mother be instructed to suspend the medicine for four hours at any time, if unusual drowsiness come on. (Dr. J. K. Spender, p. 137)

---

## AFFECTIONS OF THE DIGESTIVE SYSTEM.

Acute Intestinal Strangulation. — *Tapping the Bowel in.* —The surgeon should no more hesitate to perform abdominal section in a case of acute internal intestinal strangulation than he would to perform herniotomy in ordinary strangulated hernia. The cases are precisely parallel. When there is an immense accumulation of flatus, before performing such a serious operation as abdominal section, we are quite justified in puncturing the bowel and letting off flatus. The danger attending this is exceedingly small. The objection that we are liable to cause extravasation of some of the contents into the peritoneum does not hold good in practice, as may be seen when a strangulated hernia is tapped to let off the flatus. Nothing escapes at the minute puncture made. If this fails the abdomen should be opened. (Mr. T. Bryant, p. 219.)

Chronic Gastritis.—We can see with the eye the effect of morphia in some cases of acute sclerotitis and iritis; why should not the same "antiphlogistic" power be exercised in other painful inflammatory affections? Morphia is a remedy of some value in chronic gastritis. Let a twelfth of a grain be given twice on the first day, three times on the second, and so on until the patient consumes from one to one and a half grains in the twenty-four hours. (Drs. Barclay and Stokes, Med. Chir. Review, Jan. p. 218.)

Constipation.—*The Pulvis Glycyrrhizæ Co., of the Prussian Pharmacopœia.*—The compound liquorice powder of the Prussian Pharmacopœia, is a very pleasant and efficient laxative in cases of simple constipation. It does not produce serous transudation, but calls into play the peristaltic action of the intestine. It is composed of the following constituents so prepared as to form an almost impalpable powder: Senna leaves, ℥vj.; liquorice root, ℥vj.; fennel seed, ℥iij; sulphur, ℥iij; refined sugar, ℥xviij. (Mr. D. Page, Practitioner, May, p. 276.)

Dyspepsia of Liquids.—There is a form of dyspepsia, extremely rare however in this country, in which the patients cannot digest liquids. The presence of any amount of fluid in the stomach causes great uneasiness. There is sometimes also dryness of mouth, with dry skin and constipated bowels, and urine loaded with lithates. The best plan of treatment is the avoidance of liquid—in fact, a dry diet. In one case, on

gently vibrating the stomach fluid was heard splashing about within it, and this sound could be always produced irrespective of any liquid recently drunk. (Dr. J. C. Thorowgood, p. 130.)

Ecraseur for the Removal of Hemorrhoids.—Nott's ecraseur for the removal of internal hemorrhoids is an instrument of some value. It is easily and rapidly applied, and removes in a right line the exact amount of tissues required. It is consequently not liable to be followed by stricture of the anus. Its use is not followed by pain, owing to the devitalization of the tissues crushed, nor by hemorrhage, because of their not being completely divided. It consists of two parallel blades coming together like a clamp. One blade has a narrow fenestra running its whole length of about three inches, and the other presents a rough edge like a fine saw, so constructed as to pass into and fill up the fenestra when the clamp is closed. There is a shoulder projecting on each side of the blades, for the purpose of crushing more perfectly the tissues operated upon, a little beyond the thin edge of the blade. If the hemorrhoid is within the sphincter, it is safest to tie a ligature in the sulcus made by the ecraseur, for fear of some secondary hemorrhage—the tissue is so compressed that a very small pedicle is left for the ligature. The part of the tumour outside the ligature should be cut away with scissors. (Dr. J. C. Nott, p. 226).

Hernia.—*The Taxis.*—The best rule to adopt in the employment of the taxis is to use chloroform at once, if there is the least difficulty in reducing the hernia. It is no use your pushing one way, and the patient involuntarily pushing the other; great injury results. Do away at once with all chance of muscular effort by means of an anæsthetic, and then if you fail operate at once. (Mr. J. C. Forster, p. 216.)

*Strangulated Umbilical Hernia.*—Interference with large umbilical herniæ of old standing is nearly always followed by death. Do not attempt to return them; they are almost always chronic, slow in formation, and exist for many years before strangulation takes place. The best plan in large herniæ, when strangulated, is to puncture and let off the flatus, but if this fails to relieve the strangulation never be tempted to expose the bowel, simply cut down to the neck of the sac, divide the structure by a herniotome, and leave the rest to nature. (Mr. T. Bryant, p. 219.)

*Reduction of Strangulated Hernia without Operation.*—Invaginate the scrotum as in Wützer's operation for the radical cure of hernia, by passing the index finger *behind* the testicle and cord up to the external ring. The hernial tumour is then

pressed downwards over the finger towards the back of the hand, so as to make the structures in the ring tense, and consequently smaller. The invaginating finger is then forced firmly upwards and outwards in the direction of the internal ring. As soon as the finger is firmly grasped, the hand should be slightly turned and the finger pushed towards the middle line. Considerable force may be applied in this way, as all the delicate structures are behind the finger, which acts mainly on the stricture. On withdrawing the finger, the hernia can usually be easily returned. The same principle is equally applicable to femoral hernia. (Dr. P. C. Smyly, p. 217.)

*Tapping a large Strangulated Scrotal Hernia.*—If of very large size, and requiring exposure and manipulation of the bowel, death not unfrequently follows herniotomy. Why not puncture the bowel with a grooved needle or very fine trocar and let out the flatus? The knuckle of intestine will at once collapse and may then be easily reduced. This has not been yet tried in actual practice, but the suggestion seems good. (Mr. T. Bryant, p. 219.)

TAPEWORM.—Turpentine acts with considerable certainty, but is apt to produce intoxication and strangury. Kousso is not easily obtained genuine, and is better therefore avoided, as it will be found uncertain in its action. Kameela, has an unpleasant griping action, but is otherwise a good remedy. The etherial tincture of male fern is the best remedy. It is efficient and free from the objections to the other remedies. For its successful use the patient should take no food after breakfast except a little mutton broth or tea, and in the evening he should take a brisk aperient. The following morning early, a drachm and a half of the liquid extract or etherial tincture rubbed up with half an ounce of mucilage, should be taken in about two ounces of milk. It is better to lie quiet for two or three hours after taking this dose, otherwise it is apt to cause nausea and faintness. At the expiration of that time another aperient dose should, if necessary, be administered, as the male fern itself does not always purge. Milk is the favourite food of the worm, and is therefore the best vehicle for the administration of the medicine. After a week's rest, this ordeal should be repeated in order to be certain that the whole of the worm is expelled. (Mr. W. Date, p. 224.)

---

## AFFECTIONS OF THE URINARY ORGANS.

CATHETERISM.—*Necessity for Caution in.*—In cases where catheterism is necessary in stout elderly men, in whom there

is reason to suspect weakness of the heart, it is advisable to enjoin the recumbent position, in order to avoid syncope, till after the removal of the cathether, if not for a longer time. A fatal result may ensue if this rule is not observed. (Mr. F. D. Lys, p. 261.)

Diabetes.—*Cantani's Treatment.*—Prof. Cantani, of Naples, bases his treatment of diabetes on the theory that the question is not so much one of increased production of sugar, as of defective combustion. This he believes to be owing to the production of a morbid form of glucose, which is incapable of being transformed into lactic acid, and therefore cannot be burned, and is passed unchanged in the urine. The heat of the body is thus maintained at the expense of the albuminates and fats. The treatment consists in an exclusively animal diet, combined with the administration of lactic acid. Lactic acid is chemically intermediate between glucose and carbonic acid, and is burned off at the lungs, so saving the important albuminates and fats. This exclusively meat diet means rigorously one of plain meat, roast or boiled, without any sauces of milk or eggs, and certainly without any bread, flour, or any vegetable matter whatever, the only seasoning permitted being salt, oil, or a little vinegar. This mode of treatment is put forth as affording relief in most cases, and as effecting a cure in a larger proportion of cases than any mode of treatment hitherto devised. (Dr. G. W. Balfour, p. 145.)

Examination of the Urine in Vesical Disease. — Never let a patient pass the whole of the urine to be examined into one vessel, but a little into one and the bulk of the liquid into another vessel. The small quantity passed first contains anything which may have been in the urethra. This is especially of service in the diagnosis of chronic prostatitis, which so much resembles stone in the pain being severe in the tip of the penis, and accompanying and following micturition. There is generally a little mucous discharge in chronic prostatitis, which, except for this mode of examination, we might think came from the bladder. (Sir H. Thompson, p. 230.)

*Blood in the Urine.*—Blood is very rarely seen in simple cystitis, but it is as common in vesical calculus as hæmoptysis is in phthisis, and is especially common after some exertion, as driving or riding. Blood from this cause is rather florid in tint, whilst blood passed from the kidney remains long in the bladder, and from contact with urine is brown in colour. Hemorrhage may also occur in hypertrophy of the prostate. (Sir H. Thompson, p. 229.)

*Diagnosis between Prostatic and Calculous Disease in an Elderly Man.*—In cases where symptoms suggest one or other of these affections, enquire whether the frequency of micturition is greater at night or in the day. If in the day it is probably calculus, because the movements of the body are accompanied by corresponding movement of the stone, and consequent increase of irritation. If in the night it is probably prostatic disease. Prostatic enlargement does not trouble a patient much in the day, and calculous disease not much at night. (Sir H. Thompson, p. 228.)

*Pain in the Bladder or Penis.*—A patient complains of pain in the region of the bladder or perineum. There is almost certainly chronic cystitis. Ask whether he feels the pain before, during, or after passing urine. If the pain is before it is because the mucous membrane is becoming uneasy in consequence of distension. If the pain is during and after passing water, and in the end of the penis, he is likely to have stone; and especially also if the pain is increased by exercise. It is almost pathognomonic of stone to have the pain in the tip of the penis. Chronic prostatitis simulates stone more than any other disease. In both the pain is at the tip of the penis. (Sir H. Thompson, p. 228.)

LITHOTOMY.—*Buchanan's Method of performing.*—The feature of this operation, upon which its success depends, is that the angle of the staff is held fairly below the central point of the perineum, consequently the deep perineal fascia, and the transverse muscle and artery of the perineum, are missed by the knife in making the first incision, while they are all divided in the operation of Cheselden. In Dr. Eben Watson's hands the mortality of these operations has been only 1 in $23\frac{1}{2}$. (Dr. E. Watson, Lancet, May 4, p. 605.)

*Median Lithotomy.*—Median lithotomy is not justifiable where the stone is upwards of half-an-inch in diameter, as the orifices of the ejaculatory ducts are very likely to be obliterated and permanent impotence produced. (Mr. W. F. Teevan, p. 247.)

*To Pass a Lithotrite.*—Keep the handle close to the patient's belly, and delay the turning movement until it is called for by the fact of the lithotrite not going any further, thus announcing that it has arrived at the turning point. The instrument ought to be oiled from end to end, and the penis pulled well forward on to the lithotrite until the beak has passed the sub-pubic arch. If the patient's prostate be enlarged, a good stiff bolster under the buttocks will make all the difference between success and failure. (Mr. W. F. Teevan, p. 243.)

*Rupture of the Prostate during Lithotomy.*—Whether we examine cases of lithotomy on the living or dead, this same conclusion only can be arrived at—that complete rupture of the prostate always occurs during extraction of even an ordinary sized stone by a limited incision, and subsequent dilatation, or what is called so. Prof. Ellis has demonstrated that there is no such thing as dilatation of the prostate, and that what surgeons call dilatation is in reality complete laceration. The practical result of this is that stones should be cut out, and not torn out. (Mr. W. F. Teevan, p. 246.)

Retention of Urine from Impermeable Stricture.—Take a catheter of medium size, make a perforation in its extreme end, and pass it down to the stricture. Then take an india-rubber ball with nozzle to fit into the end of the catheter, squeeze it so as to drive out the air, and connect it with the the catheter. The patient must now be directed to make a very gentle effort to pass his water, and whilst he does this the ball is to be allowed to expand gently. The surgeon and patient will be agreeably surprised to see the urine flow through the catheter. This does not of course always succeed. (Dr. P. A. O'Connell, p. 251.)

*Retention of Urine from Enlargement of the Prostate.*—In most of these cases the retention first comes on in consequence of holding the water too long in a railway journey, or at some public meeting. Men advanced in years should be instructed to wear some apparatus by which the necessity for so long retaining the urine is obviated, when about to be placed in the circumstances named. (Mr. R. Quain, Medical Times and Gazette, May 18, p. 568.)

Squire's Vertebrated Prostatic Catheter.—This instrument is like an ordinary silver catheter in appearance, but the last three or four inches is composed of a series of perfect joints, so as to allow of a large amount of flexibility. Thus the instrument is sufficiently flexible to accommodate itself to any curves of the prostatic portion of the urethra, but it cannot double upon itself as a flexible gum-elastic or caoutchouc catheter sometimes does. At the same time it possesses longitudinal stability, that the propulsion of the hand may be transmitted without loss to the beak. This instrument has been very much approved of in New York. (Dr. T. H. Squire, p. 253.)

Stricture-Dilator.—*Mr. Hill's New Wedge-Shaped.*—A considerable amount of force is sometimes required to push up the dilator of Holt's instrument. If, instead of a circular dilator involving a good deal of friction resistance, the

rupturing force is wedge-shaped, great increase of power is obtained, with increased facility in applying it. Mr. Berkeley Hill's dilator, as used by him at University College Hospital, consists of a split sound which equals the calibre of a No. 2 or No. 3 catheter. The halves of the sound can be separated by passing between them a wedge fixed on a slender stem. The instrument is most effective and will split the hardest strictures with ease. (Mr. B. Hill, p. 250.)

URINE.—*Alkaline Phosphates in.*—To determine the amount of phosphates present in urine as alkaline phosphates, add to a portion of the urine ammonia in excess; the white precipitate consists of alkaline-earthy phosphates; filter and add ammonium chloride and magnesium sulphate; the white crystalline precipitate indicates the amount of phosphates which was originally present as alkaline phosphates. (Dr. J. C. Brown, p. 144.)

*To determine the Amount of Lime Salts in Urine.*—To a certain amount of the urine add ammonia, and filter; then add ammonium oxalate; the white precipitate contains the calcium as oxalate. (Dr. J. C. Brown, p. 144.)

VARICOCELE.—The object of treatment of varicocele is the obliteration of the mass of veins of which it consists; i.e. when the case is so bad as to require operative interference at all. Mr. Wood, of King's College Hospital, recommends continuous elastic wire pressure for this purpose, the elasticity being obtained by means of the instrument used. The instrument consists of two limbs united by a strong spring at one extremity. One limb ends in a thin round steel shaft, joining the limb at right angles, and terminating in a transversely oval and obliquely placed eye. The loop of wire having been made to surround the veins is passed through this eye, and the free ends are twisted round the end of the other shaft, which is first made to approximate its fellow. The spring will now cause constant tension upon the wire surrounding the veins. (Mr. J. Wood, Brit. Med. Journal, Sept. 16, p. 316.)

[The ordinary plan of obliterating these veins is by passing two or three small hair-lip pins underneath them, and winding a figure of 8 ligature round the free ends of the pins. EDS.]

VESICAL CALCULUS.—*The Prevention of.*—As nineteen out of twenty calculi have uric acid for their basis, practically the problem is, how to prevent the formation of this calculus. Prior to the actual formation of stone there is usually a period in which the patient habitually passes gravel, and this

without any error of diet, and incurable by any correction of diet. The ordinary plan of treatment, viz., the exhibition of alkalies in large doses is bad, as merely masking, by solution, the excessive formation of lithic acid, without remedying the cause, consequently on giving up the remedy the disease generally returns. Vichy and Vals waters are alkaline, and are not suitable. What is wanted is to stimulate the organs which are really at fault, viz., the liver and digestive organs generally, and not the kidneys, which are already doing their best. For this purpose the mineral waters of Friedrickshalle or Carlsbad are suitable. The chief constituent of these is sulphate of soda. (See table at p. 237). These waters may be procured at home, and a short course of Carlsbad, followed by a longer course of Friedrickshalle, will cure most cases of persistent lithic acid deposit. Of course a fair amount of exercise in the open air should be taken, the skin being sufficiently protected. Promotion of the action of the skin is very necessary. Alcohol, saccharine and fatty matters should be excluded as much as possible from the diet. (Sir H. Thompson, p. 230.)

---

## AMPUTATIONS, DISLOCATIONS, FRACTURES, AND DISEASES OF BONES AND JOINTS.

Amputation at the Lower Part of the Thigh.—It is always well to make a circular division of the muscles at the bottom of the wound in order that the large nerves may be divided high up, and consequently that the bulbous enlargements which occur at their extremities may be out of harm's way. If this is not done a painful stump is liable to result. (Mr. J. C. Forster, p. 177.)

Caries of the Hip-Joint.—Out of 126 cases of excision of the head of the femur for caries, 71 recovered with more or less useful limbs. A great proportion of these would have otherwise terminated fatally. The objection to the operation, that it is useless removing the head of the femur because this does not remove the carious state of the cotyloid cavity, is not in practice proved to hold good. The acetabulum, however, is not invariably diseased, for out of 81 cases it was healthy in 18. (Mr. H. Hancock, p. 165.)

Dislocations of the Femur.—A proper knowledge of the ligament which passes from the anterior inferior spinous process of the ilium to the anterior intertrochanteric line of the femur, is of immense importance in understanding dislocations of the hip, and consequently in successfully reducing

them. This ligament is of great strength, almost equal to that of the tendo Achilles. It may be called the Y ligament, because, like that letter upside down, it bifurcates into two portions, not however widely separated. It is this ligament which restrains the movements of a dislocated femur, and regulates the position of the limb. A careful consideration of this ligament shows that dislocations into the sciatic notch should be reduced by extension with the thigh bent at right angles to the pelvis. For this purpose a special apparatus has been invented, see woodcut p. 173. Where this is not at hand the surgeon should place his foot unbooted on the patient's pelvis and then lift from the knee. (Dr. Bigelow, Mr. J. Hutchinson, p. 172.)

Endoscope in Military Surgery.—Dr. Fenger, of Copenhagen, in 1869, made some experiments with the endoscope on horses, and came to the conclusion that pieces of cloth in wounds, or bullets driven into bones, could be seen by its means. Dr. Fenger has stated that during the late war he was enabled in several instances, on examining wounds some weeks after they had been inflicted, to see their interiors distinctly by means of the endoscope, without causing pain, hemorrhage, or any subsequent irritation. (Mr. T. Longmore, p. 187.)

Foreign Bodies in Wounds.—*Lecomte's Stylet-pince for the Detection of.*—This instrument consists of a central steel rod, on which slides a slender canula. The rod terminates in two small branches, each of which has a small cup-like blade or curette at the extremity. These curettes have very fine and sharp edges, which meet and overlap, forming a smooth steel knob when the canula is pushed forward, but which open wide when it is slightly withdrawn. This instrument is one of the greatest utility, as it brings away a small fragment of any substance over which the edges of the curettes glide in closing. The glistening appearance of a minute scale of lead thus removed strikes the eye at once. This instrument responds as an indicator with even more distinctness than the Nelaton probe, in all cases in which that test would be of service, while it answers for a variety of other cases in which the Nelaton probe would give no indication at all. (Mr. T. Longmore, p. 194.)

Fractures of the Femur.—*The American Splint for.*—This is a very useful apparatus. It consists of a long external splint reaching from the axilla to some inches below the foot, and a shorter internal splint extending to the same distance below the foot. The two splints are connected

together at the bottom by a thick cross-piece of hard wood, perforated by a female screw, through which works a male screw terminating in a couple of hooks. Of course the two splints are padded internally, and have soft cushions at their upper extremities. A loop of plaister is now made below the sole of the foot, the five or six long straps of which it is composed passing upwards and downwards along each side of the leg, and retained in position by other transverse pieces of plaister. The hooks are passed through the loop below the sole of the foot, and the splints bound together by straps of leather passing round them both and the included limb. A perineal band is now applied as in the usual long splint. Now by means of the screw the limb can be extended as much as is necessary. No bandages are required, and the thigh is always open to inspection. (Mr. B. W. Richardson, p. 162.)

Setons in Strumous Diseases.—Nothing does so much good in the various forms of tubercular disease as a free discharge of pus: the system seems thereby relieved of morbific matter. This is seen in the application of a seton in strumous ophthalmia. This remedy will generally prove effectual, and is sometimes our only means of arresting the disease. Even in such a disease as chronic tubercular meningitis a free discharge of matter from a large scalded surface has been known to at once arrest the disease. A seton was introduced in the chest in a case with a large cavity in the left lung, the right universally crepitant. There had been several attacks of hemoptysis. For six months a continuous discharge was kept up from setons, with so happy a result that fifteen months later she was attended in confinement, and lived six years after. (Dr. E. Crossman, p. 197.)

Spina-Bifida.—*Antiseptic Treatment.*—Dr. Wilson, of Glasgow, relates a case of spina-bifida which he treated thus: The tumour being moistened over with carbolised oil was opened by a free longitudinal incision, under an antiseptic veil of surgeon's lint soaked in carbolised oil. The lint was removed, and a large piece of carbolised lac plaster applied, and over this another, kept in position by adhesive plaster, one edge being left comparatively free for escape of fluid. A soft folded handkerchief was laid over all. A good firm pad of skin resulted. (Dr. J. Wilson, p. 193.)

Wounds and Abscesses.—*A Substitute for Lister's plan of Dressing.*—The carbolic or antiseptic system of Lister so utterly fails, when any of the precautions recommended

is neglected, and consequently involves such care and time that it becomes impossible to practice it in the out-patient department of a large public charity. The following is a good substitute for it. Wash the wound with tepid water, then with a lotion composed of equal parts of methylated spirits and water. A pad of lint dipped in the lotion is then to be laid on the wound, and in the case of a large abscess partly introduced into the same. A piece of gutta-percha tissue or oil-cloth is to be placed on the lint or tow, and a common bandage applied. (Mr. T. Cooke, Practitioner, May, p. 284.)

*Disinfectants and Antiseptics to Wounds.*—There is a difference between an antiseptic and a disinfectant, and this difference is of some importance in surgery. An antiseptic, as carbolic acid, will prevent decomposition, but does not neutralize and render innocuous the contents of a wound already in a state of decomposition. This is done by a disinfectant, the best of which for use in surgery is chloride of zinc. Chloride of zinc acts as an antiseptic and prevents decomposition, or as a disinfectant, and removes it if present; it is also not itself poisonous, like carbolic acid, nor is it volatile, and lastly its effects are not fugitive as are those of Condy's fluid. The fact of chloride of zinc solution being innocuous and non-volatile, renders a solution of it more easy to use and more generally applicable than one of carbolic acid. No complicated system of dressing is required. (Mr. C. Roberts, Lancet, April 27, p. 570.)

---

## AFFECTIONS OF THE SKIN, ETC.

ANTHRAX.—*Treatment by Strapping.*—The growth of an anthrax may be arrested, or at any rate considerably curtailed, by firm strapping with strips of plaster. These should be two feet long, each two inches wide at one end, and one and a half at the other. They are to be applied in pairs, the broad ends being first attached at opposite sides of the anthrax, in such a manner that when the narrow ends of the plaster are drawn together they shall pass over the tumour. The anthrax will require dressing in this manner once a day. (Mr. G. A. Gloag, Brit. Med. Journal, Feb. 3, p. 126.)

ECZEMA.—The dilute yellow nitrate of mercury ointment is a better local application than oxide of zinc ointment. Let anyone dress two patches with the two ointments respectively, and he will soon see the difference. But the nitrate ointment should be well made. Much of the ointment made is worthless, being dry, dirty, green, and rancid, spoiling almost as soon as it is diluted, whereas it ought to retain its bright

yellow colour for months. The dilute ointment should only be prepared when it is wanted, by being rubbed down with a little sweet almond oil, until it is the consistence of thick cream. If the eruption is seated on the head in children, the hair should be cut off, and the ointment applied night and morning. A linen cap should also be worn day and night. (Mr. J. L. Milton, p. 318.)

External Acute Inflammations.—*Tartrate of Antimony.*—Tartar emetic is of unrivalled efficacy in the treatment of some external acute inflammations, as erysipelas, and inflammation of the breast. But it is generally given in too large doses, fifteen drops of antimonial wine every hour being sufficient. Some inflammations are more curable by morphia or opium, and these are generally characterised by the predominance of pain or nervous irritability. Well marked pyrexia should suggest antimony in the doses stated. (Dr. J. K. Spender, Med. Chir. Review, Jan. p. 224.)

Ringworm in Schools.—Dr. Tilbury Fox reports that in an epidemic of ringworm, in a large school, he found the dust of the room to contain abundance of spores of tricophyton, the fungus of the disease. If therefore the air of such a room be not thoroughly disinfected by burning sulphur in it, the disease is liable to break out again. Recent cases may be at once checked, and often cured by simple blistering. If the disease has reached the bottom of the hair follicles, epilation should be practised. A good plan is to keep the head soaked in a solution of sulphurous acid; of course a proper cap of silk should be worn. The head should be washed each day. The question frequently has to be decided whether a child is well, and fit to return to school: the best guide is the presence or absence of short broken-off hairs; if these exist the disease is not cured. The hair ought not to be dull and dry, and the scalp should be free from scurfiness before the child is allowed to return. (Dr. T. Fox, p. 315.)

Skin-Grafting.—The first and perhaps only essential point is that the surface of the sore on which the graft is placed should be healthy. The piece of skin grafted should be small, and should include only the upper layer of the true skin and rete mucosum, but not the fat beneath the skin. For the removal of this the best instrument is the skin-grafting scissors of Mr. Macleod. The fragment of skin should be placed on the thumb nail, and divided into three or four pieces, and then placed about half-an-inch or three-quarters from the margin of the sore, and about one inch apart. They should be covered with a piece of oiled gutta-percha skin, and this supported with cotton-wool and a bandage. The

surface of the sore will require daily cleaning, by a stream of tepid water squeezed from a sponge. Small pieces of skin, as recommended, do better than large ones. (Mr. T. Bryant, p. 188.)

STINGS OF BEES AND WASPS.—Inject into the puncture a solution of carbolic acid. The pain will instantly cease. (Dr. A. E. McRae, p. 323.)

VARICELLA AND VARIOLA.—*The Correlation of.*—Variola and varicella are more intimately related than has been hitherto admitted by pathologists. Cases have occurred of variola being communicated from varicella. (Mr. F. T. Porter, p. 85.)

WHITLOW.—*Carbolic Acid.*—After freely slitting up the parts and giving vent to the matter and sanious discharge, use a solution of carbolic acid freely. The wound will rapidly assume a healthy character. (Dr. A. E. McRae, p. 322.)

## VENEREAL DISEASES.

GONORRHŒA.—*Carbolic Acid.*—The following is an excellent injection in gonorrhœa; carbolic acid, eight grains; tannic acid, eight grains; glycerine, half-an-ounce; water to one ounce. (Mr. G. Ashmead, p. 324.)

[We can bear testimony to the value of this combination, but have not employed it quite of the strength recommended by Mr. Ashmead. EDS.]

SYPHILIS.—*Iodides of Ammonium and Sodium.*—It sometimes happens that in cases of later secondaries, or tertiary syphilis, requiring the prolonged use of iodide of potassium, the drug ceases to produce any result, or is no longer tolerated. This may be possibly owing to the deteriorating action of the base on the blood. Ammonium and sodium are not open to the same objection, and iodides of either of these bases should be substituted. (Mr. B. Hill, p. 325.)

## AFFECTIONS OF THE EYE AND EAR.

ACUTE SCLEROTITIS AND IRITIS.—*Morphia.*—Apply a compress of lint dipped in warm water over the eye, which is to be kept closed, and give the patient the twelfth of a grain of morphia every hour for the first twenty-four hours (omitting as usual the eight hours of the night), and one-tenth, or one-eighth of a grain, every two hours for the subsequent two or three days. The patient will scarcely be aware that he is taking a narcotic. He should be restricted in diet and an occasional purgative should be given. The vascular congestion will

rapidly disappear, and the pain and photophobia diminish. The case may often be cured in four or five days. (Dr. J. K. Spender, Med. Chir. Review, Jan. p. 220.)

AMAUROSIS.—*Hypodermic Injections of Strychnia.*—In a case of amaurosis the opthalmoscope showed dulness of the media, anæmia of the disc, retina pale and anæmic. Total amaurosis had existed for nine months. The twenty-fourth of a grain of strychnia was injected, and the next day, the patient could count fingers, and distinguish colours. A continuance of the treatment led to almost complete recovery of sight. (Mr. A. S. G. Jayaker, p. 310.)

*Amaurosis from Smoking.*—There is undoubtedly a form of amaurosis produced by tobacco smoking. Excessive smoking will not in some people have any such effect, for this deplorable result is due to some extent to an idiosyncracy. It fortunately happens that there is sufficient warning of the failure of sight, and if the habit is resolutely broken off complete recovery results. (Mr. J. Hutchinson, p. 308.)

CATARACT.—*Rate of Progress of Senile Cataract.*—Those cases which commence with fine stripes in the edge of the lens while the nucleus remains clear are usually very slow in their progress, so much so, indeed, that the patient often outlives his cataract. Broad stripes in the cortical part with accompanying opacities of the nucleus are on the other hand often extremely rapid in progress. (Dr. H. B. Swanzy, p. 286.)

*Extraction of Cataract.*—There are certain disadvantages in Graefe's operation for the extraction of cataract, especially the necessity for iridectomy, with its attendant risk of prolapse of the vitreous humour; and the necessity of performing it upon the upper part of the cornea, in order that the enlarged part of the pupil may be covered by the upper eyelid. During the whole operation the eye has to be kept open by a speculum, and drawn down by the forceps. This is both painful and injurious. The operation recommended by Mr. Liebreich is as follows: the section of cornea is to be made in the lower third by means of a small Graefe's knife. The puncture and counter-puncture are to be made in the sclerotic about one millimeter beyond the cornea, the whole remaining incision passing with a very slight curve forwards, so that the centre of the flap made is about one millimeter and a half distant from the margin of the cornea. Iridectomy is not necessary, and the only instruments required are the Graefe's knife, and a cystotome with Daviel's spoon at the other end of the handle. This operation is so easy that any one, without special practice, can perform it. (Mr. R. Liebreich, p. 277.)

*Extraction by a Peripheral Section of the Iris.*—Dr. Taylor, of Nottingham, removes cataract by making a section of the iris close to its upper edge of attachment. A small piece of the periphery of the iris is then excised, and the lens extruded through the section in the iris. It is necessary to open the capsule when the first section is made. The great advantage claimed for this operation is its not interfering with the pupil, which remains uninjured and central. (Dr. C. B. Taylor, p. 284.)

Chronic Conjunctivitis.—*Nitrate of Silver.*—The best strength for a solution of nitrate of silver is ten grains to the ounce. Mr. Liebreich at St. Thomas' Hospital relies almost exclusively on the daily application of this solution in the treatment of chronic inflammatory affections of the lids, and the superficial structures of the globe. (Mr. R. Liebreich, p. 277.)

Conical Cornea.—This otherwise incurable affection may be treated with favourable results by removing the top of the cone. Before doing this a fine needle, armed with silk ligature is to be passed across the cornea from side to side. During the operation this presses gently against and supports the iris and crystalline lens, and after removal of the cone the ligature is tied in front, so as to close the opening in the cornea. The cone of cornea is to be removed in its entire thickness, and the edges of the incision are to be directly across the thickness of the cornea. A cataract knife may be used for this purpose. The cornea is thrown into slight folds by the tightening of the ligature, but these subsequently disappear. The disadvantage of the operation is, that an opaque spot in the centre of the cornea results, but none of the patients (nine in all) complained of this, the sight being otherwise so much improved. (Mr. C. Bader, p. 272.)

Glaucoma.—We must not forget to distinguish between the two forms of glaucoma—the simple and the inflammatory. The simple form is exceedingly slow, often lasting two or or three years. It is unattended by pain, and the only external signs of the disease are a few rather engorged episclerotic veins. The symptoms are increased tension of the globe, defect in vision all round the periphery of the field, the central vision remaining pretty good. Inflammatory glaucoma rarely extends over a term of more than a year, and usually but a few months. The tension advances by fits and starts, and is attended with severe ciliary neuralgia; there is also dimness of the whole field of vision. An attack of this kind generally subsides after a few hours,

and the media, which during its continuance had been cloudy, again become clear. The attacks however recur again and again, with increasing severity and frequency, until the sight is completely destroyed. No remedy is of any use except iridectomy, and this only under the two following conditions: 1. The portion of iris must be excised to its very periphery. 2. It must be a sufficiently wide piece. (Dr. H. R. Swanzy, p. 300.)

*Unrecognised Acute Glaucoma.*—Cases of acute glaucoma are not unfrequently misunderstood by men who certainly ought to have recognised their nature. Out of sixty-seven cases, fifty-two were allowed to go on until the eye had suffered serious injury from delay. The most prominent symptoms are rapidly increasing far-sightedness and the appearance of haloes round lights in the evening; then occurs superficial inflammation of the eye, an ovoid dilatation of the pupil, stony hardness of the eye-ball. The last symptoms are cloudiness and roughness of the cornea,. with loss of vision. The proper and only remedy is iridectomy. (Mr. E. Hart, p. 296.)

Polypus of the Ear.—After removal of polypus of the ear the best caustic for application to its place of growth is chloro-acetic acid, applied on a very small camel's hair brush. In using this care should be taken not to touch the meatus; and provided this is done very little pain is felt. Potassa-fusa and chloride of zinc are unmanageable, and nitrate of silver is not sufficiently powerful. When polypi exist along with a perforation of the membrana tympani, as is generally the case, it is very necessary after removal of the polypus to keep the cavity of the tympanum free from secretion, and by means of appropriate treatment to induce a more healthy condition of its lining membrane and that of the Eustachian tube. A very good lotion for this purpose is one of sulphate of zinc and opium, and with this the external meatus may be filled, the affected side being uppermost and horizontal. The patient should then blow through the perforation, and a little of the fluid will pass into the Eustachian tube on his ceasing to blow. If a polypus is large, Wilde's snare armed with fine gimp is the best instrument for its removal. (Mr. W. R. Dalby, p. 312.)

Purulent Ophthalmia in Children.—In the treatment of this disease, careful cleansing of the eyes is most important. This should be carried out throughout the whole course of the affection. Cold should be applied to the closed lids for about three days, and then the application of nitrate of silver is to be commenced. It is no use applying a weak solution of silver, and pure nitrate of silver is too strong. A mixture

of one part of nitrate of silver, with two parts of nitrate of potash, melted together, and run into a small iron mould is the proper thing. Or the end of an iron wire may be dipped repeatedly into the melted mass, until on cooling a sufficiently thick layer of caustic adheres to it. Each lid should be everted daily and rapidly wiped over with this once a day, and as the case improves every two or three days. (Mr. R. Leibreich, p. 262.)

Strumous Ophthalmia.—In a bad case of this kind, the only treatment admissible is an antiphlogistic one, consisting in the instillation of a dozen drops or so of atropine solution (gr. iv. to ℥j.) in the course of the day, with purges of calomel, and restraint in a dark room. The child should not be allowed to lie on its face. The excessive spasmodic contraction of the lids (blepharospasm) is productive of much harm. The following plan was adopted by Von Graefe, and never fails, if carried out properly to correct this. The child is raised up by two nurses, one supporting the trunk and hands, the other holding the legs, while the surgeon takes charge of the head and dips the face under water, holding it there for about ten seconds; the patient is then allowed to take a breath, and the same manœuvre is repeated several times. The effect is magical, the blepharospasm disappears, the child lies on its back, allows the eyes to be easily examined, atropine to be instilled, and becomes in every way as docile as could be wished. This process may require repetition as the spasm returns. The best topical application is Pagenstecker's yellow ointment, the composition of which, and mode of preparation, is given in the article from which this is taken. (Dr. H. R. Swanzy, p. 265.)

Substitute for the Opathalmoscopic Mirror. An ordinary watch-glass reflects quite enough light to illuminate the posterior part of the eye sufficiently for observation. The observer of course looks through it. (Dr. J. S. Torrop, p. 311.)

White Atrophy of the Optic Nerve.—Dr. Fraser, of Paisley, reports a case of white atrophy of the optic nerve, produced by over-taxing the eyes, in weaving by gaslight. He applied the continuous galvanic current, placing the electrodes on the temples. They were tried in other situations, but this was found to be the best. Each time the current was passed the sight, as proved by test types, was found to be improved, the gain in visual power being usually three inches, one of which however only was permanent. Considerable improvement in the retinal circulation also took place. (Dr. D. Fraser, p. 304.)

## MIDWIFERY AND THE DISEASES OF WOMEN, ETC.

ANÆSTHETICS IN UTERINE OPERATIONS. — *Chloroform and Bichloride of Methylene.*—Chloroform is very objectionable on account of its occasionally causing vomiting during the operation. Ether is troublesome. Bichloride of methylene is the best anæsthetic, sickness after it being rare. (Mr. T. S. Wells, p. 374.)

BLACKBEE'S RESILIENT SKELETON SPECULUM.—This simple and useful instrument will be at once understood by reference to the woodcut at page 375. It is the cheapest speculum there is, and offers the most extensive view possible of the entire circumference of the vagina and cervix uteri permitting the easy application of remedies to their entire surfaces. It is very easy to introduce on account of its resiliency, and is self-retentive. (p. 374.)

ENTRY OF AIR INTO THE VAGINA.—Air only enters the vagina in one position of the body, viz., in the prone. In all other positions the walls of the vagina remain in apposition, and this is much favoured by the movements of respiration. The knowledge of this fact is of use in examination by means of the duck-bill speculum, for by placing a patient in the semiprone position, the falling forward of the abdominal viscera is favoured, air fills the vagina, counteracts the effects of respiration, and thus enables us to get a good view of the os uteri. (Dr. R. Barnes, p. 367.)

FIBROUS TUMOUR OF THE UTERUS.—Is it wise to only partially remove a fibroid polypus when we cannot get at the whole? Undoubtedly it is. The more however is removed the better, and the less the subsequent hemorrhage. In one case in which this was done not a trace of tumour could be detected nine months afterwards, the whole of the remaining part having been absorbed or come away as discharge. (Dr. T. Skinner, p. 344.)

FIBROUS TUMOUR OF THE WOMB.—*Hemorrhage.*—The best topical application is the small solid stick of anhydrous sulphate of zinc. It can be easily introduced quite up into the uterine cavity and is far less dangerous than liquid injections. A mixture of ergot and peracetate of iron or iron alum may be given internally. (Dr. A. Meadows, p. 349.)

HYSTERIA.—Hysteria requires two factors for its production, viz., a predisposing nervous state and some local determining cause, such as uterine or visceral disorder. The predisposing state may be so strong as to produce the disease without any second cause whatever. No doubt hysterical emotions are actually conceived in the brain, but the brain sym-

pathises with the unhealthy state of other parts of the body. The local exciting causes are generally uterine or ovarian, but not invariably so. Of uterine causes it is generally the milder sorts, that are mucous membrane deep, which cause hysteria; and sometimes, by applying nitrate of silver to an ulcerated cervix, we most unwittingly bring on an attack of hysteria in patients who presented no previous sign of a tendency to it. Of visceral affections the most important exciting cause is that state of liver and stomach derangement which we call biliousness. The recurrence of hysterical attacks are only prevented in these cases by such measures as are best calculated to prevent biliary derangement. (Dr. E. J. Tilt, p. 371.)

Medicated Vaginal Pessaries.—It is no part of the function of the vaginal mucous membrane to digest fats; for this reason gelatine and glycerine in the proportion of one part of the former to four of the latter is preferable as a basis for medicated pessaries to cocoa butter which is generally employed. (Dr. A. Meadows, p. 349.)

Menorrhagia and Leucorrhœa.—*The value of Arsenic in.*—Arsenious acid is a remedy of considerable value in cases of menorrhagia, not depending upon some such cause as polypus, but simple hyperæmia of the uterus. It should be commenced in doses of two to six drops of liquor arsenicalis and very gradually increased. It is a good plan to give it in granules, each containing one milligramme, in which case the commencing dose is one to three granules three times a day. Arsenic has a decongestive action upon mucous membranes, and this renders it of great use in cases of too frequent recurrence of the catamenia with excessive discharge, and leucorrhœa in the intervening periods. These symptoms are generally accompanied with a good deal of debility. The first effect of the arsenic is to cause return of the appetite; and in two or three weeks the patient will be found to have improved in appearance and increased in weight. Very gradually the intercatamenial period will be lengthened, two or three days being gained each time, and the flow will be less abundant. The remedy should never be given in large doses, but for a considerable time. (Dr. J. H. Aveling, p. 360.)

Ovariotomy.—*Ligature and Return of the Pedicle.*—It does not appear probable in theory, and the result of experience proves it, that no harm results from tying the pedicle with silk and returning it into the cavity of the abdomen. The portion of the pedicle on the distal side of the ligature being surrounded by *warm* tissues does not lose its vitality. A case is related in which this practice was perfectly successful. (Dr. J. C. Gooding, p. 351.)

Puerperal Fever.—*The Nature of.*—Puerperal fever is not the result of a simple purulent absorption, nor of inflammation of an ordinary type extending from the genital organs, but is a true diphtheritic affection. In most cases a diphtheritic deposit may be detected on the vagina, or in the small lacerations of the os uteri. In other cases it is confined to the site of the placenta or the upper part of the uterus, when of course it is invisible to the eye, but its existence may be detected by the microscope in the discharges. In true diphtheria it has been proved by microscopic research that the spores of the fungus penetrate to the blood vessels, whence they are carried over the whole system, producing a general disease. It has not actually been proved that a similar process takes place in the case of puerperal fever, but the probability is so great as almost to amount to certainty. (Dr. E. Martin, Berlin, p. 335.)

*Puerperal Septicœmia.—Carbolic Acid.*—It appears from a case related by Dr. Trask, of Astoria, U.S., that carbolic acid may be administered so as to impregnate the system with it. The result in this case was that the signs of septicæmia "were kept in abeyance." The acid was administered by the mouth, rectum, and vagina. A half drop of Calvert's solution was given in mucilage by the mouth every two hours. The solution for the rectum was one drop, increased to five drops to an ounce of mucilage, a half gill being thrown up after every dejection. There was a good deal of diarrhœa. The vaginal injection was not less than five drops to the ounce every three or four hours. Within twenty-four hours the diarrhœa ceased, and the case commenced to improve to some extent. The patient tasted the carbolic acid in everything. (p. 341.)

Stricture of the Cervical Canal of the Uterus.—The operation of division of the os internum and cervical canal by a hysterotome, as practised by Sir James Simpson, has now been on its trial for a sufficient length of time to permit of our judging of its merits. Dr. Protheroe Smith states that, although it gives relief at the time the original constriction is increased by it. He had one case in which two ridges of cicatricial tissue existed corresponding to the two lateral incisions made many years before. As a substitute for incision, Dr. Smith has for many years practised dilatation by means of an instrument terminating in two blades, after the model of Heurteloup's lithotrite. The use of this instrument is contraindicated if there is any inflammatory condition, whether of the mucous lining or of the deeper tissues of the uterus. If hyperæmia exists it is best reduced by repeated scarifications of the labia uteri laterally at the commissures. He considers that the results of dilatation are more permanent than those of incision. (Dr. J. P. Smith, p. 353.)

# PRACTICAL MEDICINE.

## DISEASES AFFECTING THE SYSTEM GENERALLY.

### 1.—THE PATHOLOGY AND TREATMENT OF PYREXIA.

By Dr. Charles Murchison, LL.D., F.R.S., Physician to St. Thomas's Hospital.

The natural heat of the body is due to vital and chemical processes resulting in the oxidation or combustion of nitrogenous and carbonaceous substances furnished to the blood by the tissues, but mainly by the food. The products of this combustion are eliminated from the lungs in the form of carbonic acid, and from the kidneys as urea and uric acid. The oxidation of carbon resulting in the formation of carbonic acid is effected by the corpuscles of the blood; whereas recent researches make it probable that the albumen is transformed into urea and uric acid in its passage through the gland-cells of the liver, spleen, and other glands, and through the cells of the blood itself. The albumen which is thus being constantly transformed or split up into urea is not the fixed albumen of the muscles, nerves, and other formed tissues; but the so-called store-albumen, which exists in the blood, and is constantly passing to the cells through the body, and returning to the blood again. From this, also, the organs take what they require, and the waste is made up partly by the effete albumen cast off by the tissues, but mainly by the food.

The preternatural heat of fever is the result of vital and chemical action exalted above the standard of health, assisted perhaps by a disturbance of the processes by which heat is carried away. The proof of this is found in the augmentation of the products of metamorphosis eliminated by the lungs and kidneys, and in the loss of bodily weight far exceeding what can be accounted for by the mere abstraction of food. We have already found that there is an increased elimination of carbonic acid from the lungs in pyrexia, which may in part account for the consumption of fat. It is, however, the increased elimination of nitrogen by the kidneys which has been mainly

studied. The amount of urea and uric acid voided in the urine is, as we have seen, largely increased in fever. There is evidence, also, that the increased excretion of urea precedes any rise of temperature; and although the amount of urea cannot be measured by the degree of heat, there is a direct relation between the two. As a rule, the temperature is highest, and the quantity of urea greatest, in the early stages of a fever; and when there is an unusual elevation of temperature, there is an unusual amount of urea. There are, no doubt, exceptions. The temperature is modified by the amount of evaporation going on from the surface of the skin; and the urea may be lessened by albuminoid matter, more or less changed, being retained in the blood. In badly nourished persons, also, it has been found that comparatively little urea is eliminated notwithstanding the rise of temperature; but the latter is also less than in the robust and well fed, and may be due to an increased formation of carbonic acid. In one respect, the source of temperature in fever differs from that in health. In health, the elimination of nitrogen is entirely regulated by the amount entering the body with the food, as has been well shown by Parkes and other observers; but the increase of nitrogen eliminated in fever does not come from the food, for it is out of all proportion to it. The fixed albumen of the muscles, brain, and nerves breaks down into circulating albumen, to be in its turn transformed into urea and other nitrogenous excreta. Hence it is that the muscles waste and the brain becomes atrophied. The large amount of cerebral fluid so common in protracted fevers is merely thrown out from the blood to fill the space vacated by the brain, and is not of inflammatory origin, as was once believed. In all fevers the muscles become wasted, and, on microscopic examination, the muscular fibres can often be seen in the act of disintegration. The granular and waxy degenerations of the muscles found by Zenker in enteric fever occur in in all severe fevers, and in typhus the cervical ganglia of the sympathetic have been shown to undergo a granular degeneration. The impaired nutrition of the heart and of the nitrogenous tissues accounts in part for the weakened cardiac action and for the stagnation of blood in the capillary circulation, which are so common in the advanced stages of fever. The only parts of the body that do not waste in fevers are the glandular organs, and especially the liver, spleen, kidneys, and lymphatic glands, which become enlarged and congested from the increased functions thrown upon them, the enlargement being greatest in the young and robust, who have most tissue to spare for conversion into urea. The gland-cells of these organs become swollen with minute granules; and a similar appearance is often presented by the white corpuscles of the blood, which are usually increased in number.

The large amount of nitrogenous detritus formed in fever may be all eliminated by the kidneys or bowels, or a portion may be retained in the blood, either as urea or as some half-transformed albuminoid matter, and then the temperature may be elevated without a corresponding augmentation of urea in the urine. Critical deposits of lithates in the urine are chiefly observed in those cases of fever where, from the symptoms, it might be inferred that there has been a retention of nitrogenous detritus in the system. The urea, or other less oxidised products of metamorphosis circulating in the blood and permeating the tissues, gives rise to symptoms of uræmic poisoning, or to the typhoid state already referred to; this condition being in some instances aggravated by the non-elimination of carbonic acid, consequent on the congested condition of the lung.

There are good grounds for the opinion that this is the true pathology of the typhoid state. 1. All modern pathologists are agreed that the cerebral symptoms of most fevers are independent of any appreciable structural lesion of the brain or its membranes. 2. The typhoid symptoms of febrile diseases often closely resemble those of uræmia resulting from disease of the kidneys. The two conditions are, in fact, often mistaken for one another; and excepting that the temperature is increased in the one case and not in the other, it may be sometimes difficult to distinguish them. The symptoms in both cases are probably due to the presence of similar morbid materials in the blood, the difference being that in pyrexia these materials are generated in excess, while in renal disease the kidneys are unable to eliminate the normal quantity. 3. In the course of these lectures I shall have occasion to tell you that in many fevers presenting typhoid symptoms—such as typhus, relapsing fever, yellow fever, and the secondary fever of cholera—urea has been actually found in the blood and in the cerebral fluid. 4. Lastly, the advent in fever of grave cerebral symptoms, and in particular of convulsions, which may be regarded as the acme of both uræmia in kidney-disease and of the typhoid state in fever, is often preceded by albuminuria, or by a marked diminution, or even suppression, of urine. When stupor, delirium, and coma show themselves in the course of a contagious fever, it is the custom to refer them to the action of the fever-poison on the brain; but the cerebral functions are more probably deranged, not by the fever-poison, which was the first and necessary link of the pathological chain, but by the accumulation in the blood of the products of metamorphosis, and the consequently perverted and defective nutrition of the brain and nerves. Hence it is that the symptoms in the advanced stages of many fevers and local inflammations (the typhoid state) are closely assimilated, although the primary

maladies have been widely different. The precise grouping of the symptoms will vary in different cases of the same disease, while in diseases essentially distinct it may be identical.

It is difficult to say why the nitrogenous detritus is excreted in some cases and retained in others; but its elimination appears to be often prevented by some morbid condition of the large glands, and especially of the kidneys, either of old standing or consequent on the febrile attack. Disease of the kidneys is a serious complication of all fevers. As long as the kidneys are equal to the increased work thrown upon them, the blood is properly depurated, and the typhoid state is warded off. But if the kidneys be unequal to the task, either from the large amount of effete material to be eliminated, from previous disease of the secreting tissue, or from congestion and other changes resulting (as they often do) from their increased work, then the blood becomes polluted, and convulsions or the typhoid state supervene. This explains why albumen so commonly appears in the urine in the course of all acute diseases of a severe form, and why it is so justly regarded as an unfavourable symptom. It also explains why albuminuria is looked upon as a contraindictation to any serious surgical operation. The surgical fever which follows an operation is attended by an increased metamorphosis of the blood and tissues; and if the kidneys be diseased, the products of this metamorphosis will be partly retained in the blood, and induce the typhoid state with all its dangers.

Another probable result of the non-elimination of nitrogenous products in fever is the development of local inflammations. The half-changed albuminoid matters circulating in the blood seem sometimes to be deposited in different organs, and there to excite secondary inflammation. Cases of idiopathic fever have been observed in which a sudden diminution in the amount of excreted urea has been followed by an attack of pleurisy or other local disease, the quantity of urea again increasing as the local complication has receded.

When convalescence from fever is fairly established, and the patient is regaining weight, the elimination of nitrogen and also the temperature are often below the normal standard.

The metamorphosis of nitrogenous matter which occurs in health is under the control of the nerves, and so the augmented metamorphosis of fever appears to be due to some abnormal state of the nervous system. According to the well-known experiment of Claude Bernard, an elevation of temperature to the extent of from 7 to 11° F. is produced on one side of the face of an animal, when the trunk connecting the sympathetic ganglia on the corresponding side of the neck is divided, the sensibility of the part becoming greatly excited and the vessels

dilated and hyperæmic. This elevation of temperature must be ascribed to the hyperæmia and increased metamorphosis in the part, which had before been held in check by the sympathetic nerve. The converse of this experiment has been performed by Waller, who found that contraction of the dilated capillaries, diminution of vascular injection, and reduction of temperature followed the irritation of the divided sympathetic by the transmission of an electric current. Experiments on the vagus nerve have led to equally important results. Section of the vagus is followed by increased rapidity of the heart's action, and an increased lateral pressure of blood in the arteries; whilst irritation of the cut nerve by an electric current reduces the number of beats of the heart and diminishes the lateral pressure in the arteries. These and other observations make it probable that the increased metamorphosis, the elevated temperature, and the increased frequency of the heart's action in fever, are due to paralysis of the sympathetic nerves and the vagus. Many clinical facts show that the nervous system exercises a powerful influence on the early phenomena of fever, such as the rigors, the pain, the languor and prostration, which are among its earliest phenomena.

The facts and arguments bearing on the pathology of pyrexia now laid before you may be summed up as follows.

1. A morbid condition of the blood, due to the entrance of some poisonous matter from without or generated within the body, or to some local injury or inflammation, exercises a paralysing influence on certain portions of the nervous system, and particularly on the sympathetic and the vagus.

2. Increased rapidity of the heart's action is one of the earliest results.

3. A second result is a rapid disintegration of the nitrogenous tissues into substances of a simpler chemical construction, while little or no fresh material is assimilated to compensate for the loss. Increased temperature, great muscular prostration, and loss of weight are the natural consequences.

4. The non- elimination, from any cause, of the products of this disintegration gives rise to cerebral (typhoid) symptoms or local inflammations.

5. The impaired nutrition of the heart itself and of the rest of the body in conjunction with the polluted state of the blood and the nervous paralysis already referred to, induces in severe cases of fever great weakness of the cardiac contraction, and stagnation of blood in the capillaries in different parts of the body.

Why it is that the febrile process once lighted up should ever become arrested, and in many instances after a definite duration, is a problem in medical science of which no satisfactory solution has yet been offered.

*Diagnosis.*—From what has been stated, you will understand that mere quickness of the pulse does not constitute pyrexia, but that its diagnosis must depend upon the presence of an elevated temperature. It is this that constitutes the great value of the clinical thermometer in diagnosis. It enables you to say positively whether a patient is suffering from fever or not. Against one fallacy in diagnosis I wish to put you on your guard. A patient may be suffering from pyrexia; and at certain hours of the day, which may correspond to those of your observations, the temperature may be perfectly normal. In any case of doubt, then, it will be necessary, before excluding the existence of pyrexia, to note the temperature at two or three different hours in the course of the day. In phthisis, for example, and in certain cases of enteric fever, the pyrexia is often for a time distinctly intermittent.

*The Prognosis* in any case of pyrexia will of course be greatly influenced by its cause; but, irrespectively of the cause, there are certain conditions which must always be regarded as of serious import.

1. In grave fevers the prognosis is worse in robust full-blooded persons with abundance of muscle and fat than in those of spare habit, because in the former there is more material to spare for disintegration during the febrile process. In scarlatina it is a matter of daily observation that the plump and ruddy child succumbs, while the spare and more delicate-looking child pulls through. And in adults it may be said that all fevers are grave in proportion to the development of muscle and fat.

2. The prognosis is always bad when severe fevers attack individuals in whom the defæcation of the blood is naturally imperfect—for example, in persons of a gouty diathesis.

3. The danger in any case of fever is increased by the fact of the patient having a naturally weak heart, not so much from the presence of valvular disease as from weakness in the contracting power of the muscular walls. Its strength may be sufficient for the ordinary work in health; but when fever occurs, the work is increased while the organ is further weakened, and then it is apt to break down.

4. The danger is always great when there is evidence of antecedent renal disease. To the kidneys we must look for the elimination of the large amount of nitrogenous detritus resulting from the febrile process; and, when the eliminative power of these organs is impaired by disease, the peccant material is retained in the system, with the deleterious consequences already described. Here again, the kidneys may be sufficiently sound for the due performance of their functions in a state of health, although they are quite incompetent to discharge the additional work thrown upon them in pyrexia.

5. In most fevers the prognosis is bad in proportion to the intensity of the pyrexia, as measured by the thermometer. With few exceptions, a case with a temperature of 105° must be looked on as severe; and one with a temperature of 107° as highly dangerous.

6. In any case of fever the prognosis will be bad in proportion to the intensity of the typhoid state; and when convulsions occur in the typhoid state, recovery is rare.

7. In any case the prognosis will be bad in proportion to the weakness of the heart, and the tendency to stagnation of the blood in the capillaries of the lungs and other parts of the body.

*Treatment.*—If the views which I have laid before you respecting the pathology of fever be correct, our objects in treatment ought to be as follows:—

1. To remove when possible, the cause on which the fever depends.

2. To promote elimination, not merely of any morbid poison, but of the products of exaggerated metamorphosis in the blood and tissues.

3. To reduce the temperature and the frequency of the action of the heart.

4. To maintain the nutrition of the tissues and stimulate the action of the heart by appropriate food and stimulants, taking care, at the same time, not to excite congestion or increase the work of the already overtasked glandular organs.

5. To relieve dangerous and distressing symptoms.

6. To obviate and counteract secondary complications.

How these indications are to be fulfilled will be discussed under the head of individual febrile diseases, but a few general remarks on the treatment of fever in the abstract will not be out of place here, and may save repetition hereafter. You are not to imagine that each fever has a treatment of its own. According to the predominance of certain symptoms, the appropriate treatment will vary greatly in different cases of the same fever, while in fevers widely different in their causes it may be identical.

1. Unfortunately, it is not often that we have it in our power to remove the cause of pyrexia; but the object is one always to be kept in view, and sometimes the main efforts of our treatment must be directed to secure it; as, for example, when pyrexia depends upon pent-up pus, an obstructed bowel, or gouty, syphilitic, or periosteal inflammation.

2. The elimination of any morbid poison, as well as of the products of exaggerated metamorphosis, will often be promoted by the judicious employment of diaphoretics, diuretics, purgatives, and emetics. The old practice of commencing the treatment of pyrexia by giving a purgative to unload the portal

circulation and promote the action of the liver, is undoubtedly a good one, and is particularly advisable in persons of robust habit, or who live too well. In mild cases of pyrexia, the only treatment necessary consists in the avoidance of any chill, and in the administration of a mild aperient, followed by frequent doses of diuretics, and diaphoretics, such as the citrate of potash, or the liquor ammoniæ acetatis with spirit of nitrous ether. Elimination will also be promoted by a plentiful supply of fresh air, which will favour the escape of carbonic acid from the lungs, and by the free use of diluents, which will help to wash away through the kidneys the products of tissue-waste. In all grave cases of fever you will remember the importance of maintaining the action of the kidneys, and of keeping a good watch on the state of the urine; noting carefully not so much its colour and the presence or absence of lithates (both of which characters will depend much on the quantity), but the quantity and the presence or absence of albumen. When the quantity becomes notably diminished, or albumen appears, advantage will often be derived from hot poultices to the loins, aperients, diaphoretics, diluents, and diuretics. But while you promote elimination, you must take care that the means for this end do not weaken too much the action of the heart; and you must remember that in some fevers, the natural processes of elimination are excessive, and conduce to dangerous exhaustion and death.

3. For reducing the intensity of the pyrexia, different measures have been proposed.

*Blood-letting* was at one time universally resorted to for this object, but in this country it is now entirely discarded, because it was found to increase one of the great dangers in pyrexia, viz., failure of the heart's action. There are few accurate observations on the effects of blood-letting on the temperature of pyrexia; but we know that, when a copious bleeding from the nose or the bowel takes place in enteric fever, although the temperature may fall to below the normal standard, it speedily regains its former height, or rises above it.

*The external use of cold water* is one of the most certain means of reducing temperature in pyrexia, and in certain cases is attended with good results. The attention which this practice is now attracting will justify the following remarks. In the seventeenth century, the brothers Hahn, of Leipzig, treated fevers by the external use of cold water, but their observations were soon forgotten. Towards the end of last century (1787) cold affusion was proposed by Dr. Currie, of Liverpool, both for arresting and mitigating fever. The patient was seated naked in an empty tub or bath, and several buckets of water, of a temperature of 40° or 50° F., were poured from a height of

one to three feet, or more, over the head and chest. He was then hastily dried, and restored to bed, and in most cases, the operation was repeated once or twice daily. It was stated that, in many cases, if resorted to during the first three days, this treatment arrested the disease ; while, in others, it reduced the pulse and temperature, relieved many of the distressing symptoms, and particularly the headache, restlessness and delirium, and conducted the disease to a safer and speedier issue. The affusions were employed at any stage of the fever ; but the effects were always most salutary at an early stage. They were said to be contraindicated when the temperature of the skin, ascertained by the thermometer, was not much above the normal standard, or when, notwithstanding an elevation of temperature, the patient complained of chilliness, or sufferred from severe diarrhœa or profuse sweating. The wonderful results obtained by Currie, were confirmed by numerous observers in different parts of the world, whose testimony is recorded in the third edition of his work, published in 1804. But in the British epidemic fever of 1817-19, the practice was followed by many with great perseverance, and the general result, according to Sir Robert Christison, was that in very few cases, if any, was the disease arrested by it ; that although an abatement of febrile heat and restlessness occurred almost invariably, it was of short duration, and not to be made permanent by any frequency of repetition ; that as much good eventually was attained by frequent cold or tepid sponging, together with cold applied to the head ; and that often the cold affusion occasioned for a time after each application an intense feeling of pressure and weighty feeling in the brain, which could not be regarded without some uneasiness. These statements, backed by professional and popular prejudice, account perhaps for the subsequent neglect of the cold-water treatment of fevers. But the observations made of late years by Brand, of Stettin, Jürgensen, of Leipzig, Liebermeister, of Basle, Ziemssen, of Erlangen, and H. Weber, and Wilson Fox, of London, show that, although the practice may not shorten the fever, and is often inapplicable, yet under certain circumstances it is useful not only for reducing the temperature, first of the surface and then of the interior of the body, but for relieving headache and other distressing symptoms, removing congestion of the kidneys, warding off delirium and coma, and rousing the nervous system in cases of excessive stupor. The circumstance has perhaps been too much lost sight of that cooling the body may not influence the conditions on which the development of heat depends ; but with reduced heat it may be assumed that there will be diminished metamorphosis, to the non-elimination of the products of which many of the dangers of fever are due.

In point of fact, Schroeder, of Dorpat, has ascertained that cold baths effect a marked diminution in the excretion of carbonic acid and urea in fever; and as this was not attended by any aggravation of the general symptoms, it is fair to attribute it to a retarded metamorphosis of tissue.

Statistics have been appealed to to prove the great success of the cold-water treatment of fevers (particularly of enteric fever) as contrasted with that of an expectant method; and, although other conditions not stated may have helped to influence the result, they suffice to show that the practice is not beset with the dangers commonly imagined. But the most conclusive facts in favour of the practice are those observed in certain cases of hyperpyrexia by Dr. Wilson Fox and others, where its employment was followed by recovery from an elevation of a temperature (110° F.) which under every other method of treatment has been speedily followed by death. At the same time there are many cases of pyrexia in which the cold effusion or immersion would be unsuitable or injurious. It is likely to be of most service when the temperature is unusually high; and in all under 102° F., or when the extremities are cold, although the cases the practice is contraindicated when the temperature is temperature of the central parts of the body be high; and it must always be employed with caution when there are the signs of weakened cardiac action or of stagnation of blood in the capillary circulation, although it may be noted that in one of Dr. Fox's patients, who was apparently rescued from death, the face was cyanotic and the radial pulse imperceptible.

There are different plans for employing cold water in the treatment of pyrexia, such as the cold affusion practised by Currie, packing in a cold wet sheet resorted to by Brand, or immersion in cold baths. The last is the method now most in fashion. The patient is placed in a bath having a temperature of from 50° to 70° F., or better, as Ziemssen recommends, in one whose temperature is about 10° below that of the body, but which, after the patient's immersion, is gradually cooled down to 68° by adding cold water. He should remain in the bath for half an hour, or until shivering comes on, and all the time he is in the bath his limbs ought to be rubbed by assistants. He is then to be hastily dried and put into a warm bed. For some time after the bath, the temperature in the rectum continues to fall as the trunk parts with its heat to the extremities; but as soon as the temperature in the rectum rises again to 104°, the patient ought to have another bath. In the early stages of the fever, as many as seven or eight baths in the day may be necessary. When cold affusion or immersion is contraindicated or inexpedient, frequent sponging of the surface with cold or tepid water will also help to cool the body, and is often a source of much comfort to the patient.

*Quinine in large doses* has an undoubted influence in lowering the temperature of pyrexia. In most cases of severe pyrexia, ten, fifteen, or twenty grains will, within an hour or two, cause a fall of the temperature to the extent of three or four degrees, and to a less degree of the pulse. It is true that the effect passes off after a few hours, and that there is no good evidence (except in malarious fevers) of its cutting short the natural course of the attack; but the effect may be maintained by a repetition of the dose; and the remedy has often appeared to me to be of signal service when a pyrexia was at its crisis, and when the temperature was rising in place of falling.

*Digitalis, Aconite, and Veratrum Viride* have a marked power in reducing the pulse, and, to a less extent, the temperature in pyrexia, and are, in my opinion, too much neglected for these objects in practice. Veratrum viride is largely used in America in the treatment of fevers, and its effect upon the pulse is speedy and most decided; the only objection to its use in private practice which my experience suggests is its liability to induce sudden nausea and faintness, but these symptoms are transient, and cease on the administration of a stimulant. Ten or fifteen minims of the tincture may be given every four or six hours. Aconite is a remedy of great value for reducing the pulse and temperature in fever, and especially in the pyrexia resulting from local inflammations, and is much less used than it deserves to be. Digitalis is another remedy which I have often found very serviceable in various forms of pyrexia. While increasing the force of the cardiac contractions, it diminishes the frequency of the pulse, reduces the temperature, and increases the flow of urine. Lastly, *antimony* reduces, in a marked degree, the frequency of the pulse in pyrexia, and promotes diaphoresis and mucous secretion. It was at one time largely used in all fevers, but in many it is contraindicated by its tendency to weaken the contracting power of the heart.

4. The nutrition of the body must be maintained by appropriate food, in the form of milk, beef-tea, eggs, and farinaceous articles. Not long ago it was the custom to starve fevers; and you may probably have heard that the late Dr. Graves, of Dublin, who was mainly instrumental in doing away with this objectionable custom, expressed a wish that his epitaph might be "He fed fevers." The modern tendency, however, is perhaps to over-feed fevers, and especially to give too much nitrogenous food. Dr. Parkes has shown that there are theoretical objections to a purely nitrogenous diet in fevers. It is doubtful if the disintegrating nitrogenous tissues can be fed; and in that case the albuminous food must be got rid of by the already overtasked glandular organs. Milk is in most cases preferable to beef-tea as an article of diet in fevers.

In many cases of fever it will be necessary to give stimulants. You must not give stimulants simply because a patient has fever. Many patients with fever do better without them. But you must not refrain from giving stimulants when the heart shows signs of weakness, as happens in the advanced stages of most protracted fevers. The heart may be artificially stimulated by sinapisms and other irritating applications to the skin, but better by the internal administration of ammonia, ethers, and alcohol, in quantities proportioned to the weakness of the heart and pulse.

5. In every case of pyrexia, you must combat dangerous symptoms as they arise. Stagnation of blood in the pulmonary capillaries impeding the aeration of the blood is to be met by stimulants, such as alcohol, carbonate of ammonia, and ethers, Digitalis, by strengthening the heart's action, and turpentine, which seems to stimulate the capillary circulation, are also useful under these circumstances; while advantage will likewise be derived from mustard- and linseed-poultices to the chest, sometimes from dry cupping of the chest, and from warm applications to the feet. When uræmic symptoms predominate, the action of the skin and bowels is to be promoted, digitalis and saline diuretics may be given to increase the flow of urine, sinapisms and linseed-poultices are to be applied over the loins; while attempts may be made to rouse the patient by cold affusion to the head, by blistering the shaven scalp with liquor ammoniæ, and by sinapisms to the nape and feet. In many cases of fever you will also be called upon to relieve distressing symptoms—such as diarrhœa, pain, sleeplessness and delirium —which, if unchecked, hasten exhaustion and prevent recovery.

6. You must counteract, as far as possible, secondary complications, which will vary according to the primary cause of the pyrexia, and which always add to the patient's danger.

Lastly, I would caution you against two errors in the treatment of pyrexia.

1. You must take care that the remedial measure which you adopt in no way thwart the natural modes of recovery, or favour the natural modes of death.

2. At the same time, you must not be content with adopting a treatment of pure expectancy. You must not forget that the natural termination of pyrexia may be death, as well as recovery.—*British Medical Journal, Feb.* 27, 1872, *p.* 175.

---

## 2.—ON THE PERIOD OF INCUBATION OF TYPHUS, RELAPSING, AND ENTERIC FEVER.

By Dr. Charles Murchison, LL.D., F.R.S.

[The knowledge which we possess upon this subject is very meagre and unsatisfactory, being based upon insufficient data.

Dr. Murchison first gives thirty-one observations on the period of incubation of *Typhus fever* and then continues:]

From the cases now recorded it would seem that the usual period of incubation is about twelve days. Of the nine cases in which it was exactly determined, in four it was twelve days; and in thirteen more of the thirty-one cases it might have been twelve days. In other words, out of the thirty-one cases, in seventeen the period of incubation was either twelve days or this duration was within the known limits. It may be added that Jacquot, who calculated the latent period from the date of embarcation of healthy French troops on board vessels infected with typhus, found in a considerable number of cases that it varied from nine to thirteen days, the average being somewhat less than twelve days.

But occasionally the period of incubation exceeds twelve days. It did so with certainty in four only of the thirty-one cases now recorded. In one only of the cases was there reason to think that it was as long as twenty-one days. Theurkauf records two cases, in one of which it was eighteen days and in the other between fourteen and nineteen days. Peacock also relates the case of a man who, in 1862, nineteen days after his admission into a surgical ward of St. Thomas's Hospital, was attacked with typhus to which it is believed that he could only have been exposed prior to his admission. I know no reliable facts, however, showing that the latent period of typhus can exceed three weeks, and statements to the effect that it can extend over several months require confirmation. Few, at all events, will admit, on the evidence adduced by Bancroft, that an interval of five or six months may elapse between exposure to the poison and the commencement of the disease, an opinion to which he was forced by his determined opposition to the possibility of an independent origin of the fever.

On the other hand, in not a few cases of typhus the period of incubation is less than twelve days. It was so in ten, at least, of the thirty-one cases now recorded. Davies records the cases of four Norwegian sailors, who on the night of their ship's arrival in Bristol from Onega, visited some typhus-fever nests, and all four sickened with typhus eight days after. In my own second attack the latent period was exactly five days (Case 20). There are also authentic instances of an extremely short latent period, or where there has been scarcely any latent period at all. The late Sir Henry Marsh collected nineteen cases in which the disease manifested itself almost instantaneously after exposure to the poison. In most of the cases the persons complained of an offensive odour proceeding from the beds or bodies of the sick, and immediately suffered from headache, great prostration, nausea, or rigors, followed by the usual

symptoms of typhus. Similar cases were mentioned by Haygarth; others were observed by Gerhard at Philadelphia, in 1836; and in two of the thirty-one cases now recorded (Cases 24 and 25) there were reasons for believing that the symptoms commenced immediately after the first exposure. In some of these cases it might be difficult to exclude the possibility of previous exposure to the poison, but in others there were no grounds for such suspicion, and in all, the patients appeared to be conscious of the moment at which the poison entered the system. It would seem that the poison of typhus may be so concentrated, or that the system may be so susceptible of its action, that its effect may be almost instantaneous.

From the above facts the following conclusions may be drawn:

1. The period of incubation of typhus varies in duration in different cases.
2. In a large proportion of cases it is about twelve days.
3. In exceptional cases it is longer than twelve days, but it rarely, if ever, exceeds three weeks.
4. In many cases (one third or more) it is less than twelve days, and occasionally there is scarcely any latent period, the symptoms commencing almost at the instant of exposure to the poison.

[He is able to offer fewer observations on the period of incubation of *Relapsing fever*; the facts which he has observed, however, point to the following conclusions:]

1. The period of incubation of relapsing fever is not a fixed period, and is even more variable than that of typhus.
2. It is, on the whole, shorter than that of typhus. In not one of the nine cases in which it was accurately determined did it exceed nine days; in none of the twenty-five was there reason to believe that it exceeded sixteen days; in only two did it certainly exceed twelve days, and in only three others was it possible for this period to have been exceeded; while in fourteen of the twenty-five cases, or in more than one half, it did not exceed five days.
3. Occasionally, as in typhus, there is scarcely any latent period at all, the symptoms commencing almost immediately after the first exposure to the poison.

*Enteric Fever—(Typhoid or Pythogenic Fever.)*—Reliable facts bearing on the period of incubation of enteric fever are even more difficult to obtain than illustrations of the latent period of typhus or relapsing fever. In my own practice I can call to mind only two cases throwing light upon the question, and in these all that could be said was that the period of incubation was not longer, in one case than twenty-one days, and in the

other than fourteen days. Medical men of much experience in fever, and among others Dr. J. B. Russell, Superintendent of the City of Glasgow Fever Hospital, and Dr. T. J. Maclagan, formerly Superintendent of the Dundee Infirmary, have also informed me that they have met with no cases showing the latent period of enteric fever. Several circumstances contribute to make it very difficult to obtain satisfactory evidence on the point as regards enteric fever :—1. The difficulty in many cases in deciding when an attack of enteric fever really commences. 2. The circumstance that nurses and patients in fever hospitals rarely take enteric fever. 3. The fact that in private practice, when the disease has been imported into a healthy locality, according to my experience it rarely spreads. 4. The difficulty often in determining, when a person is seized with enteric fever soon after changing his residence, whether he has brought the disease with him, or whether he has been predisposed to the disease by recent arrival in an infected locality.

From the facts before us the following conclusions may be drawn:

1. The period of incubation of enteric fever is most commonly about two weeks.

2. Instances of a longer duration appear to be more common than in typhus or relapsing fever.

3. The period of incubation is often less than two weeks, and, as in typhus and relapsing fever, it may not exceed one or two days.

It would be an interesting inquiry how far the period of incubation varies according as the poison is introduced by the alimentary canal or by the lungs. — *St. Thomas's Hospital Reports*, 1871, *p.* 23.

---

## 3.—ON THE DIAGNOSIS OF TYPHOID FEVER IN ITS EARLY STAGES.

By Dr. P. W. Latham, Physician to Addenbrooke's Hospital, Cambridge.

[In typhoid fever cases the thermometer is indispensable. The physician who judges of fever cases without it may succeed pretty well if of large experience, but he is to some extent in the dark, and will not treat his patients so satisfactorily.]

During the first four or five days the general symptoms which may then, as I have told you, accompany the disease—viz., the rigour, the langour and feebleness, headache, epistaxis, giddiness, pain in the back and aching of the limbs, the appearance of the tongue, the state of the bowels, the condition of the urine, &c.,—may not be very distinct, or any one of these morbid symptoms may be entirely absent. In a consider-

able number of cases, in fact, it would be impossible for you to say, without using the thermometer, whether the patient were suffering from typhoid fever or not. But the thermometric course of the disease at this time, unless it supervenes on some other malady, is very regular; and by taking the temperature at 8 a.m. and 6 p.m. for three days the presence of typhoid fever may be decided. On the other hand, one single observation may, with very great probability, negative the existence of the disease.

The following is the formula (from Wunderlich) of this initial stage:—

| | Morning. | Evening. |
|---|---|---|
| 1st day .. .. | 98·6° F. .. .. | 100·4° F. |
| 2nd ,, .. .. | 99·4° .. .. | 101·4° |
| 3rd ,, .. .. | 100·4° .. .. | 102·6° |
| 4th ,, .. .. | 101·6° .. .. | 104° |

If, then, a person, previously quite well, feels uneasy, perhaps has a rigor, and in the evening we find his temperature about 100·4° or 101° F., falling the next morning about a degree, rising again in the evening, and approximately following the above course, the disease may be diagnosed with tolerable certainty.

On the other hand, the disease is not typhoid fever if (1) on the second, third, or fourth evening the temperature approximates even to the normal (98·6° F.); (2) if during the first two days the temperature rises to 104° F.; (3) if between the fourth and sixth days the evening temperature of a person under middle age does not reach 103°; (4) if the temperature on two of the first three evenings is the same; or (5) if it is the same on the second and third mornings. From the fourth to the tenth day the evening temperatures are tolerably uniform, the highest being most generally on the evenings of the fourth, fifth, or sixth days, and reaching from 104° to 105·5° F., or even higher. The morning temperatures are from 1° to 2·6° F. lower than the evening ones; on the fifth, sixth, and seventh days the variations between the morning and evening temperatures being less than take place from the sixth or seventh to the ninth or tenth days. During this period (from the fourth to the tenth or twelfth day), if the general symptoms are obscure, an absolute diagnosis may be readily made, and the disease may be confounded with several others, unless thermometric observations extend over several days. Here, again, the thermometer gives us then very definite information. Wunderlich tells us: "That if in a youth or individual of middle age, who was previously well, an illness has continued from five to ten days, and we find evening temperetures of 103·4° to 105° F.,

or a little over, alternating with morning temperatures of 1·4° to 2·6° lower, and without any other disorder showing itself to explain the elevation, and without the patient previously being subject to gross neglect, then we are justified in positively diagnosing the disease as typhoid fever." There is at the present time in the hospital a girl in whose case the application of this law materially helped us to arrive at a correct diagnosis; from the ninth to the twenty-first days of the disease, as you see from the chart, her evening temperatures mostly reached 103° to 103·8° F., with some morning temperatures 1°, and some 2° lower. This, excepting small successive crops of rose spots, was the only distinct evidence of the existence of the disease; the other symptoms—the condition of the tongue, the character of the evacuations, &c.—not being at all characteristic. The normal temperature in this case was not reached until the thirty-fifth day.

To Professor Wunderlich's work on temperature I am much indebted for the information it affords. I last year in my clinical lectures gave you copious extracts from it on febrile diseases in general, on pneumonia, ague, &c. It is soon to appear in an English form, translated for the Sydenham Society, and I recommend it to your careful study from the great importance and extreme interest of the subject.—*Lancet, March* 2, 1872, *p*. 287.

---

## 4.—ON THE ORIGIN OF FEVER.

By Dr. Pratt.

[The following paper was read before the Surgical Society of Ireland.]

I have read a great deal lately in the newspapers, both English and Irish, as well as in the weekly medical periodicals, relative to the cause of typhoid and other fevers. The writers abstaining in general from the production of any real facts, seem to be unanimous in attributing such diseases, in their inception, to the decomposition of animal and vegetable matter. They trace the first rise of the malady to malignant effluvia emitted from manure heaps, stagnant pools, drains, sewers, cesspools, and to all such heterogeneous accumulations as are found near the dwellings of the poor and farming classes, as well as to the gases arising from the closets and closed sewers of the rich, who dwell in palaces and fare sumptuously every day.

After an experience of a quarter of a century as an Irish Dispensary Medical Officer, I venture to record my dissent from hese conclusions. As a practical man, in charge of a large and

populous district, together with a practice over 200 square miles, I need not say that I have had abundant opportunities of becoming familiar with the dwellings and habits of all classes of the community; and it is my firm conviction that the agencies above indicated cannot be actually productive of fever of any type. Were it otherwise, Ireland would ere this be depopulated from sea to sea, or at most but sparingly and thinly inhabited.

It is a well-known fact that, with rare exceptions, the farm yards of the Irish agricultural classes are simply the open spaces either in the front of their dwellings or close behind. The office houses, the cow houses, stables, &c., either ranging with the main dwellings, or actually forming a component part of them. The farm yard manure, carelessly heaped, in many instances up to the very doors, and in such a way that it often becomes a problem to the perplexed doctor, whose practice is desired within, how to effect an approach, especially when called upon in the dead of night, without sinking ankle deep in mire and filth, or perhaps coming to worse grief in the shape of a souse in the slough of despair.

Such is the state of affairs during the winter months—in the hot weather of the summer the pits, from which the accumulated mauure had been removed to the farm, serve as a receptacle for slops and refuse of all sorts thrown from the houses. These slops, fermenting in warm weather, produce a green stagnant pool, the gases generated showing themselves in the form of bubbles on the surface, which in due time burst, and, of course, discharge their *supposed* noxious contents in the immediate neighbourhood of the dwelling, with all its inmates, old and young.

In places such as I have very inadequately described, I may say I have rarely observed a case of fever of any type to occur. The average length of life is very high in many such places, and I may say also, that illness in many such places is almost unknown, save in the form of a common cold, or such diseases as are peculiar to infantile life and childhood.

In many instances, I am sorry to say, our peasantry live in a state of barbarism even worse than that already described—the farm stock being housed in the same dwelling as the human inmates, the manure thrown in heaps everywhere about the house up to the very threshold; the pigs and poultry fed upon the kitchen floor, and the refuse of the repast, together with the contributions from the guests—porcine and feathered—swept into a deep pit close by the fire-side (called the ash-hole or ash-pit), which is rarely cleaned out until it is filled to overflowing. Yet, even in such habitations as these I have found the families hale and sound—strangers to fevers and other diseases which,

if the contrary received theory be correct, ought to be too familiar acquaintances.

Let me add an illustration:—Some eight or ten years ago, during the construction of the Newry and Armagh Railway, the small town of Markethill was crowded to excess with navvies and their families, there was not an available spot in the town, but was filled to suffocation—many rooms tenanted with two or more families at night. Notwithstanding all the filth and stench consequent upon the over-crowding, the season was one exceptionally free from disease generally, and in particular marked by the absence or rare occurrence of those of a febrile character.

Nor is my experience in these matters at all singular in what I have stated in reference to the continuance of vigorous health in the close neighbourhood of influences commonly believed to be of a noxious character. Clergymen of all persuasions meet with the same state of things in their walk of life, as well as Dispensary Medical officers, and the intelligent members of the Constabulary Force who, being brought by their statistical duties into close connection with the farming classes, are perhaps qualified to give an opinion as well as clergymen and doctors.

It is a fact that vegetable and animal life will flourish and thrive wonderfully in the vicinity of such places as I have already pointed out: grasses of all sorts grow luxuriantly around the borders of such places, and ducks and many sorts of waterfowls feed and fatten upon such stagnant pools, with their bills as deep into them as they find necessary to procure feeding from the bottom, oftentimes as far down as length of neck will permit.

Rats and frogs also live and thrive well about such places. I have recently seen a closed sewer, which runs through a farmer's yard, opened for the purpose of having it cleared out; it had not been opened for many years previously; it was almost entirely filled with most offensive smelling matters. After the removal of the flags, which formed the covering, a number of frogs were found lodged together at one side of the drain, more than two yards from the opening into it, where they had collected to hybernate; they were all alive, and had every appearance of health, and no doubt, were they permitted to remain a little longer time, they would have come forth from their retreat, of their own accord, in due season, to spawn.

I carefully examined the spot where the frogs collected themselves, and was unable to discover the remains of a dead one, thus showing that the gases generated in the sewer had no injurious influence upon them.

Familiar as I am with facts such as I have described, I own it has seemed to me passing strange to see it so confidently asserted that His Royal Highness the Prince of Wales must have caught the dire disease, which has so lately been the cause of so much anxiety to every loyal heart in the realm, from defective sewerage in the house of his noble entertainer.

"The wind bloweth where it listeth;" and in my humble opinion it is the same with the seeds of disease.

Strange, indeed, are the laws of epidemics: at one time, small-pox rages, as at present; at another, fever; at another, scarlatina; at another, erysipelas; at another, *cholera*; whitlow, perhaps, and influenza, &c. Let it be *particularly noted* that the conditions productive of fatal disease are, in all cases, subject to little or no variation, and therefore, where the causes are perennial, as among the Irish peasantry, there ought to be no respite from the ravages of deadly maladies.

The facts, unfortunately for the theory, are perverse; instead of a confirmation, they supply a refutation, to my mind irresistable.

The conclusion to which all my experience tends may be expressed briefly, and will, no doubt, have been anticipated by my audience, viz., that the causes generally assigned as generating typhoid and other fevers are completely inadequate, and that it yet remains to be discovered from what mysterious sources those fatal maladies arise.

Dr. Darby said he could corroborate every word of Dr. Pratt's paper by his own observation. It was most valuable, because of the theories that were now put forward, not only as to contagion or infection spreading from the diseased to the healthy body, but also with regard to its production from manure heaps and decomposing organic matter. There was only one cause which he thought would account for the spread of disease from human being to human being, and that was overcrowding. All his life he had seen the healthiest human beings reared in farm-yards, the children playing from morning till night on the dunghill, and the pigs and fowls running about the house. The more filth about a farm the richer a farmer would be. His object was to make manure, and he did it, but without detriment to the health of the inhabitants of the farm. A few months ago he was attending a case of small-pox in the dirtiest house he knew. It was filled with children; it was filled also with cocks and hens, and he was not at all sure that the pig did not sleep under the bed. He had the curiosity to measure the room in which six children were sleeping. It was seven feet wide by nine feet long, and six and a half feet high, and had but one small window. Six children were sleeping in one large bed in that room, and one of the

children had small-pox. He recommended the removal of the child to hospital, but the parents would not consent, He then told them to remove the other children from the room, but this could not be done, as there was no other place to put them. This was three months ago, and not another case of small-pox occurred in the house. The overcrowding of people in small lodging houses, did, he believed, generate fever, but he denied that manure heaps or gas emanating from decomposing matter would do so. Practically, he thought the wisest course was to leave a vent for the gas to escape and to prevent its accumulation, and in some places that was done; but in truth, he did not think the disease called typhoid fever could be traced to any distinct source. In these farm-yards and houses the people went on from year to year with the same dirt, and there was no small-pox, typhus, typhoid, or scarlet fever among them. A change took place, an epidemic set in, and then disease spread among people who had been free from it for years. Some gentlemen said these diseases could be stamped out. He doubted the possibility of this being done. They did not stamp out the cattle disease, or the grape disease, or the potatoe disease. They were fighting with an unknown enemy, and they were not prepared with the proper weapons to fight him. He did not mean to disparage cleanliness—quite the contrary. He believed it possible to stamp out endemic disease by improving the habits and increasing the comforts of the people. They might stamp out scrofula and cachexia by sanitary and other measures, but he did not think they could stamp out epidemic disease.

Dr. STOKES was glad to be present at the reading of Dr. Pratt's paper. He was the first to bring forward the important fact connected with the prevalence of fever in Ireland, that, while the presumed causes of fever are permanent, the effects are not permanent but intermitting, or at least remitting. The dung heaps, the imperfect sewage, the filth of the country farmers' places formed a permanent institution. The occurrence of fever was epidemic and inconstant. That was one great fact not easy to get over in the theory of the septic origin of fevers. He remembered a good many years ago when there was a tremendous epidemic of fever in certain parts of Ireland, and especially the southern portions; and when Limerick, Cork, and other towns were the centres of the most terrible plague, the town of Killarney might be said to have escaped. The Grand Jury of Kerry, greatly struck with this, directed his brother, who was County Surveyor, to make a survey of Killarney, which he did, and the result was something startling. It was proved that there was, by some enormous proportions, a greater quantity of putrescent matter in Killarney than

either in Cork or Limerick. The number of cubic feet of putrescent matter to each individual was something incredible. A good deal had been said of the distinction between typhus and typhoid fevers. He should be sorry that that Society adopted those hard and fast lines of distinction between different forms of essential disease. They were all very well on paper and in the books of nosologists; but the practical man who met with these diseases in great numbers, would tell them that there were many cases which he could not put clearly into this category or that category. Many of the authors who contended for these hard and fast lines of distinction, had not observed or practised in this country. Their experiences had been drawn from the London or Edinburgh Hospitals, and they had not practised much, or at all, in Ireland. If they did they would have an opportunity which did not occur in English Hospitals, of seeing an entire family in the wards of an hospital at the same time, both the parents and five or six of the children; and in that group of cases, where one cause most probably produced the disease, they would meet with every possible variety of fever, well marked petechial typhus, non-petechial fever, pythogenic fever (to use the newest phrase), or enteric fever, simple fever, rheumatic fever. Therefore it seemed a very difficult matter to say that these diseases are essentially distinct. He had lately read a paper on the "Correlation of Zymotic Diseases." He would go a step further and say, it might yet be a question whether the various cases of zymotic disease were not only correlative but convertible. With respect to the question of stink, the tendency of modern sanitarians was too much confined to the removal of nuisances. They thought they would do all that was required by preventive medicine. Preventive medicine was a development of knowledge of our own generation, and promised to be one of the most glorious products of the present time—far beyond curative medicine, for that depended on the slow advance of medical science, which all depended on the characters and the training of the professors of curative medicine. But preventive medicine implied the cure of the masses of mankind and of the population at large. It depended on wise legislation. He believed the sources of epidemic diseases were not yet known. Dr. Pratt gave utterance to the same idea when he said "the wind bloweth where it listeth." The great Humboldt, in his Cosmos said, that among the various questions that involve human inquiry, there was none more obscure than the origin of epidemics. It was a fashionable doctrine that endemic and epidemic diseases could be removed or destroyed by what was called sanitary reform. The argument on which the doctrine was based was this: an epidemic

disease appears: sanitary measures are adopted, and the disease disappears. But this argument was really a weak one, when they reflected that epidemic diseases were like diseases in individuals; they had this appearance, their growth and their spontaneous subsidence, and the question was whether this subsidence was the result of sanitary measures or merely the result of a general law. The greatest disgrace had come on the profession of medicine, by gentlemen putting forward remedies for this disease and that disease, that would disappear without any remedy at all, and attributing the recovery of their patient to the last remedy applied. And so it was with epidemics. They could not then necessarily attribute their cessation to the adoption of this or that curative remedy or preventive measure, or sanitary reform. There was another matter which Professor Macnamara had not alluded to, but which he knew was in his mind, for he had frequently spoken to him about it—the great error of attributing great phenomena to a few limited number of causes or to one cause. They only knew of one cause capable of producing great phenomena, and that was the cause of all the phenomena of the world and of man—he meant God. They did not know of any great phenomena or series of phenomena that was not the offspring of an infinite number of causes; and to say that typhus results from emanations proceeding from the overcrowding of human beings, or that pythogenic fever proceeds from the putrescence of matter in which the seeds of typhoid fever had been mixed, would seem altogether wrong. Dr. Graves, when there was an outbreak of Cholera in London, wrote his celebrated article against the view of the *Times*, in which he showed the folly of attributing it to the sewage. He thought it was settled that bad odours were not in themselves necessarily deleterious. It was quite possible that one odour might be deleterious and another not. Labourers working in knacker's yards and in sewers did not appear to be more liable to disease than other persons in the same class of life. Dr. Pratt, coming from the North of Ireland, was well aware of the frightful odour from the maceration of flax that pervaded the country at certain seasons, yet it had never appeared that it had been the cause of an outbreak of typhoid or any other fever. With regard to sanitary reform, civilization required that all these deleterious influences should be removed—not merely bad smells, but everything, whether of a moral or a physical nature that could retard the healthy development of the human frame. They should inquire whether sanitary reform acted simply by extinguishing the germs of disease, or whether it acted by the improvement of the general health of the population, thereby enabling the human body to resist contagion,

or, if it yielded, to bear up well against the disease. He believed that this was the true way of looking at sanitary reform.—*Med. Press and Cir., March* 6, 1872, *p.* 206.

---

## 5.—THE ORIGIN OF FEVER.

By Dr. EDMUND P. SHARKEY, Ballinasloe.

It occurred to me when reading the discussion on Dr. Pratt's paper before the Surgical Society of Ireland, [see preceding article] that he and those who adopted his view, did so without considering how difficult it is to prove a negative, inasmuch as that one well-authenticated fact will outweigh the most plausible opposing theory. The same thought must have occurred to other readers as well as to me, to many of whom, I doubt not, cases in their own experience must have suggested themselves confirmatory of the views sought to be impunged. It has so happened that I have at this time under observation what I consider a crucial instance. The case I allude to is the following:—A large family lived in a house in one of the parishes composing my dispensary district—a house long notorious for its filthy surroundings: the whole frontage was faced by a cesspool, the receptacle of all the filth which the proprietor, in the ardour of his manure manufacture, could rake together from every quarter. It was situated in a hollow, and the house and offices being higher, and there being no sewer, of course the rain and other moisture could not escape. His landlord, a very kind and generous man, frequently offered to assist him by *shoring* up the house in order to make a passage for the liquid portion of the collection; but in vain. The consequence was that a more than usually offensive atmosphere pervaded the dwelling. I doubt if my friend Dr. Pratt, in his Connaught experience, ever encountered so bad an one—I know I never did—and while writing an order for food for the patients I was obliged to beat a hasty retreat from the premises. Well, though the family constitution long held out, it at length gave way; the father was attacked by a low form of fever, which I have been in the habit of designating "typhoid" but which lacked two of the characteristics *now* included in the term—viz., enteric complication and rose rash. He with great difficulty struggled through it, though more than once thought to be moribund. When he became convalescent, the oldest daughter took it, and others in succession, so that at last there were six lying ill, four in one small room (the father's); but none of them had it so severely as the father. Now, I may observe that there had not been a case of fever in the parish for weeks or months, that the father had not been near any one

sick during the same period any where else, and that a family of three persons, relatives of these people, who had visited them during the couse of the fever, took ill; the father of this second family, a fine young man, a ploughman, died of the worst form of the disease, marked by black tremulous tongue, delirium ferax, followed by coma vigil, and, at the close, convulsions. This last was the only one in whom the rash was observed, but there was no enteric complication. Now, here, was a fever of a bad type, for whose origin, in my mind, no reason can be given save one,—the odour of putrefying matter, animal and vegetable. I may observe that the well from which they drank was a very fine one, far away from the cesspool. With regard to the second family, it may be doubted whether their illness arose from infection or malaria. I think the former, and if I am right, it confirms Dr. Stokes's assertion "that we cannot adopt the hard and fast lines of distinction between typhus and typhoid disease, however well they may look on paper and in the books of nosologists." Here were several cases presenting different types, all originating in the same source, a source now generally believed to be the origin of the typhoid type of disease, yet without the characteristics marks of that type, and seemingly highly infectious, contrary to the received opinion regarding that type. I cannot help thinking, from the views advocated by speakers of eminence on that occasion, that Medical opinion is not, after all, very progressive. I remember, when young, hearing an anecdote of an old-fashioned and eccentric physician in Cork, who said, at a meeting of the Profession on the occasion of the fearful epidemic to which Professor Stokes alluded, "that he did not think there could be a more wholesome thing at a man's door than a fine dunghill." He was thought then to be not *au courant* of Medical science, and now we find his sentiments repeated more than half-a-century after, in a meeting embodying the highest Medical talent in the country. I agree with Drs. Stokes and Pratt that "stink" *per se* will not produce fever, but all effluvia are not deleterious in proportion to their offensiveness, for I am free to confess with one of the speakers that the smell from flax pools is even worse than that from the ordinary dung heap. As to Dr. Pratt's difficulty of understanding "how the greater part of the Irish population should not have perished, if these emanations were morbific," he must remember the Irish cabins are generally built with two "hall doors," through which the wind freely plays, and when it blows strongly in front the front one is closed, and when at the back the back door; so that the tainted air is blown over and round the house, or to speak poetically, "'Tis odour fled as soon as shed." Again, as to his argument about the luxuriant verdure

of the herbage about its brink, that it must be equally favourable to animal life, he might as well say that the Sunderbunds of Bengal, noted for its luxuriant jungle, could not produce jungle fever or cholera, whereas it is well agreed that it furnishes the most lethal forms of both.—*Medical Press and Cir.*, *March* 27, 1872, *p*. 283.

---

## 6.—ON THE TREATMENT OF THE FEBRILE STATE.

By Dr. Lionel S. Beale, F.R.S., Physician to King's College Hospital.

[In considering this question it is necessary to discuss the influence of remedies upon the feverish condition generally, irrespective of the causes which have given rise to it. It will appear that there are certain general facts of the highest importance, the proper estimate of which will lead to the adoption of certain measures of great practical value in all forms of fever.]

Acting upon the hypothesis that the danger to life from fever depends upon the increased temperature, many Physicians have adopted plans of treatment which have for their object the carrying off heat from the surface. This is accomplished by the external application of cold. Although there is evidence that cold to the surface does good in some cases in which the body-heat is above the normal standard, few Physicians feel sufficiently satisfied that conclusive proof has yet been adduced that this indication for treatment can be with wisdom fearlessly followed out in all cases.

This remarkable increased development of heat in all fevers is probably an immediate effect of another change. It is certainly associated with, and (I believe) due to, very rapid growth of bioplasm, principally in the slow-moving blood in the capillaries; but the bioplasm in the tissues external to the capillaries is also involved, and in some fevers in a very remarkable degree. The increase of the bioplasm continues, as does also the development of heat, for an hour or two (and in rare instances for a much longer period) after death, and occasionally up to a certain time even in an increased ratio; and for this reason: The bioplasm being perfectly still, and everywhere surrounded by nutrient pabulum, is under the most favourable conditions for appropriating rapidly all that lies sufficiently close to it therefore rapidly increases until the adjacent pabulum is exhausted. But when this happens, as no fresh nutrient fluid can possibly be brought to it, the bioplasm dies, and the temperature soon falls.

Now (as will be presently explained), there is reason to think

that many of our remedies act beneficially by the direct influence they exert upon the process of increase of the bioplasm in the blood and in the tissues. In fever and certain low states of the system, the bioplasm of the tissues and blood invariably increases more rapidly than in health; and, as I showed many years ago, the classes of remedies which experience has proved to be beneficial are those which check the growth of bioplasm. Stimulants effect this change quickly, and many of the so-called tonics possess this property in a very remarkable degree though they act more slowly.

Cold reduces the temperature, and may also be instrumental in bringing about a state of things unfavourable to the growth of the bioplasm, which is the immediate cause of the rise. If, however, we could prevent the undue growth of the living matter, or check it before it has proceeded to a dangerous degree, the temperature might not rise so high as to render the external application of cold either necessary or desirable. If we proceed back yet another step in the inquiry, we shall find that the multiplication of the masses of germinal matter or bioplasm is due to the rapid appropriation of excess of pabulum. If, therefore, this pabulum were removed, or its constitution changed, or its formation prevented, there is reason to think that the increase of bioplasm would be stopped, in which case the feverish state could not be established. Or, if the blood could be urged more quickly through the capillaries, the heat developed would be carried off, and pass away latent in vapour from the skin and lungs. The rise in temperature would not take place so long as the increased rate of the circulation compensated in this manner for the undue development of heat. This remark will appear strange to many readers who attribute increased body-heat to increased oxidation; but there can be no doubt that the view is correct, for the temperature invariably rises as the capillary circulation becomes more feeble, and, as already remarked, often obtains its maximum some time after the blood has ceased to circulate at all—when, in fact, the functions of respiration and circulation, which are specially concerned in oxidation, have ceased.

In *health* any disturbing influences are compensated with remarkable rapidity. Thus, exposure to degrees of heat or cold differing considerably from the body-temperature will produce very slight change in the internal temperature. The same thing is observed as regards alcohol. In the experiments of Parkes and Wollowicz, moderate doses of alcohol appeared to influence very little the temperature of the body or the amount of nitrogen excreted. Traces of alcohol escaped, but there can be no doubt that the larger proportion was appro-

priated by the bioplasm of the blood, and afterwards eliminated in the form of excrementitious matters, especially carbonic acid and water. But in fevers and inflammations the mechanism by which this process of compensation is carried out undergoes deterioration or is seriously damaged, so that artificial efforts are required to put it into operation at all. In health it works perfectly well without the introduction of any stimulus from without.

The beneficial effects of external cold in febrile conditions have been long known, and cold was practically employed in the treatment of fever by Currie so long ago as 1789.

Of late years the more careful employment of the thermometer in disease, as proved to be of great value, especially by the observations of Traube, Wunderlich, Ringer, and others, and now carried out by the great majority of Practitioners, both in public and private practice, has, as might have been anticipated, led to a revival of the treatment of fevers by cold. In order to form a correct judgment concerning the influence of cold upon the phenomena of fever, I would refer to the results which have been observed to succeed its application in cases of extreme severity, and I doubt if any more to the point could be selected than those which have just been published by Dr. Wilson Fox.* I desire more particularly to call attention to these cases, because they also afford an illustration of the free use of stimulants in the very last stage of desperate fever, at a period of the disease when the temperature was so high that a fatal termination was imminent; indeed, it is almost certain that death would have occurred had not the most decisive measures been promptly taken.

Excessive and very rapid rise in temperature is perhaps seen more frequently in acute rheumatism than in any other febrile disease. In very severe cases of this affection it is not uncommon to find a rise of 4° or 5° within a period of a few hours, death usually taking place a short time after a temperature of 107° has been reached, and sometimes before so high a maximum has been attained. The high temperature may persist for some time after death, and a further (post mortem) considerable elevation has been witnessed. It is, indeed, seldom that recovery occurs after the temperature has reached 106° in a case of acute rheumatism; but Dr. Fox gives a full account of two remarkable cases of the disease which recovered after a temperature of 107·3° and 110° respectively had been attained. Both these cases were treated by the application of cold. In the first, when the temperature was 107°, the patient was immersed in a bath at 96°. The temperature, however, still rose until it was 110° in the rectum, when "ice was fetched; a large

* "Treatment of Hyperpyrexia." Macmillan and Co. 1871.

lump was placed on her chest, another on her abdomen; a bag filled with ice was tied down the length of her spine; and while two assistants bailed the warmer water out of the bath, two others poured iced water over the patient as rapidly as the pails could be filled." *Within half an hour the temperature in the rectum had fallen to* 103·6°, *and in less than another half-hour to* 99·5°. It is, however, worthy of note that in this instance the patient took *six ounces of brandy during the time* (one hour) that the ice and cold water were being applied, when the temperature fell more than 10°. Subsequently eighteen ounces of brandy per diem were given for several days.

In Dr. Fox's second case, also, in which the temperature was reduced from 107° to 98° within an hour, large quantities of brandy (from twenty-four to twenty-eight ounces in twenty-four hours) were given; and Dr. Fox expresses a doubt whether the man would have recovered at all had the stimulant been withheld.

In a case of Dr. Meding's,—quoted by Dr. Fox—the temperature fell from 108·6° to 99·5° *in five hours* during the application of ice-cold cloths to the body, and enemata of iced water given every half-hour. The pulse fell from 140° to 72°, perspiration ensued, and the patient rapidly recovered. In this case no stimulant at all was administered, but the temperature fell much more slowly than in Dr. Fox's cases. It was certainly slight as compared with the two cases reported by Dr. Fox, and many that I have myself seen, and treated successfully with frequently-repeated doses of brandy without recourse to the application of cold.

Dr. Wilson Fox considers that in acute rheumatism active treatment by cold should be commenced when the temperature reaches 107°, and all Physicians who have had much experience in the treatment of desperate cases of the kind will be disposed to agree with him; for although the opinion may be fairly entertained that in many cases the rise of temperature to the danger-point may be prevented by treatment, and particularly by the administration of alcohol, it appears to me to have been fully proved that cold (with or without stimulants) is the only remedy yet tried by which the rise in temperature can be not only quickly checked, but decidedly and rapidly reduced after the high body-heat of 107° has been reached. In moderately severe cases many will doubt if there is any necessity for adopting the treatment by cold; and it is certain that many most serious cases have progressed favourably, and have recovered, under other plans of treatment, and especially under the influence of alcohol. But in such very serious instances of disease as those reported by Dr. Wilson Fox, there can be no doubt that the application of cold should be resorted to. The

objections that can be made to the plan are, in my opinion, far outweighed by the circumstance that, if not adopted, the patient will almost surely die, and within an hour or two.

By merely reducing the temperature of the body, however, we do not remove the causes which have given rise to the increased development of heat. The rise in temperature is not the *cause* of the feverish condition, but a concomitant effect, or a consequence of a prior change—the increase of bioplasm. At any rate, in every one of the cases of fever that I have examined, both in man and animals, I have invariably found very considerable increase in the proportion of the bioplasm of the blood and tissues. If the bioplasm did not increase, there would have been no undue development of animal heat—no fever. This, then, seems to be the change of which the febrile or inflammatory state is an immediate consequence, and it is therefore of the utmost importance to consider carefully the various means at our disposal for preventing, or modifying, or cutting short the conditions which favour the occurrence of such a morbid change, or which may modify the intensity of the latter when it has occurred. It is also highly desirable to discuss the circumstances which, in severe forms of disease, occasion the fatal result, either suddenly or more gradually, by inducing structural changes which render impossible the continuance of life.

In all fevers and extensive general inflammations there is more or less failure of the capillary circulation, and the danger varies according to the degree of the disturbance, and the extent of tissues and important organs involved. In very severe cases, what we have to apprehend, and what our greatest efforts should be directed to avert, is *stagnation of blood in the small vessels, and cessation of the capillary circulation over a considerable part of the body.* To bring about this result, the following circumstances contribute :—1. Failure of the force of the heart. 2. Alteration in the composition of the blood. 3. Growth of the bioplasm of the blood, vessels, and tissues.

In very severe cases of fever, what we have to apprehend, and that which our greatest efforts should be directed to avert, is *stagnation of the blood in the small vessels, and cessation of the capillary circulation over a considerable part of the body.* To bring about this result, the following circumstances contribute:

1. Failure of the force of the heart's action.
2. Alteration in the composition of the blood.
3. Growth of the bioplasm of the blood, vessels, and tissues.

1. *Failure of Heart's Action.*—In many severe attacks of fever the danger to life depends upon the weakness of the heart's action; and of those who succumb to fever, not a few are known to have had a weak heart, and that degree of power

as regards the ganglionic nervous system which, although consistent with prolonged, steady, and equable work, is liable to fail if the demand for greatly increased work should arise. The cardiac mechanism may be well adapted for the ordinary requirements of the system in health, but, nevertheless, unable to bear a strain, and quite incompetent to discharge double duty even during a short period of time. There are many who are capable of performing steadily and constantly a moderate amount of labour without suffering, and may perhaps continue to do so, and without being laid up for even a short time, and may even reach in fair health the period of life when the power of active labour ceases, but who would be immediately deranged and in danger of being destroyed, if a double or treble amount of work were suddenly thrown upon them. In all cases of fever it is important that the Physician should endeavour to form an estimate of the heart's healthy power, for by good management we may be able to keep steady the force of the circulation throughout the whole period of the malady. During a critical period it may be necessary to excite the organ to increased action; and at another time it may be desirable to pour in nutrient food instead of stimulants, and thus slowly renovate the strength. There are cases in which, by the failure of the heart to propel the blood with sufficient energy through the capillaries, the patient's life is for a time in great jeopardy. By timely and active measures, we may succeed in exciting the heart to more vigorous action, and thus the patient may be successfully carried through a very dangerous period of the malady. In all cases of fever, it is important to watch very carefully for any indications of decided failure of the heart's action; but, at the same time, it is very necessary not to modify the treatment at every slight change that may be noticed in the force of the heart.

In slight febrile affections, the heart's action is usually sufficient for the work it has to do during the fever. If the feverish condition lasts more than a few days, and the heart contracts feebly, its vigour may often be restored by the administration of small quantities of easily digested food at very short intervals of time. Two or three teaspoonfuls of milk or strong beef-tea every two hours during the night as well as during the day may be given, and a little wine may be ordered if food alone does not have the desired effect. But if, in cases of fever which continue for a fortnight or more, we find the heart's action becoming decidedly weaker, it is necessary to stimulate artificially, at least for a time. A stimulating action is produced by certain remedies which act through the nerves. Stimulating liniments or turpentine externally will sometimes have the effect. Remedies which stimulate the olfactory and

respiratory portions of the mucous membrane act in the same way; but the most direct as well as the most efficacious means is by the introduction of stimulants into the stomach; and these also act beneficially in other ways. Ammonia, chloric ether, and various kinds of spirits (when given not too much diluted) produce an immediate though indirect influence upon the force of the heart's contraction. This is proved by the fact that very soon after a little wine, brandy, or ammonia has been taken when the heart is acting very feebly, as during a partial faint, or from the influence of chloroform inhalation, the vigour of the heart's contractions becomes sensibly increased. The change is often noticed within a few seconds after the stimulant has come into contact with the mucous membrane of the stomach. This action, no doubt, depends upon the influence produced upon the ganglia of the sympathetic transmitted by afferent fibres from the skin, (mucous membrane) of the nose, throat, respiratory passages, or stomach, as the case may be. In consequence, the blood is urged with more force through the capillaries of the body generally, and the tendency to death from this cause is postponed, and perhaps averted.

2. *Alteration in the Composition of the Blood.*—In this place we have to consider how we may prevent the textures and organs of the body being damaged, and life destroyed, by the altered blood circulating in the vessels; and also to inquire whether we can, by judicious interference, promote and accelerate the restoration of the nutrient fluid to its normal state.

It was formerly supposed that the chief thing to be guarded against in the treatment of fever was undue oxidation; but later investigations have proved that fever itself is due indirectly rather to changes consequent upon insufficient oxidation; and it is, indeed, probable that a febrile state may be engendered by long-continued insufficiency of this most necessary change. It is doubtful if the feverish state could exist in an organism in which the oxidising processes were performed perfectly and at a proper rate. There is good reason to think that if the noxious materials which accumulate in the blood in fevers and extensive internal inflammations could be more fully oxidised, they would soon afterwards be eliminated in the form of urea, carbonic acid, and other excrementitious matters which at last result from the destruction of bioplasm and the oxidation of the products of its death; but while these excrementitious matters, or the imperfectly oxidised materials from which they are immediately produced, continue to accumulate in the blood, the excreting organs cannot eliminate them or alter them fast enough. The latter organs may in many instances be excited

artificially to increased action; and it is in this way that many diuretics, sudorfics, and purgatives often afford great relief, and restore the patient to health. But in serious cases, in which the strength is already much exhausted, more especially if the fever has yet a long course to run, there would be danger in pushing too far the use of such remedies. Moreover, the excreting organs cannot excrete some of these unoxidised animal matters. In not a few instances in which the noxious materials have unduly accumulated, it will be found that the secreting organs *cannot be made to act;* as the afferent nerves are in part paralysed, our remedies are powerless. To give excessive doses in such cases would be very unwise, and by so doing we should render the condition worse. Under the circumstances indicated, however, there is danger of the imperfectly oxidised substances accumulating in the blood and in the tissues to such an extent as to place the patient's life in great jeopardy. And the danger is twofold. In the first place, many of the bodies are unstable compounds, and liable to decompose at the temperature of the body. The products of decomposition set free in the blood would very soon destroy the living matter of the blood and tissues, and paralyse and destroy the nerve-fibres and nerve-centres, in which case the patient must die. Secondly, if the compounds which cannot be excreted in the form in which they exist are not to undergo decomposition, they must be quickly taken up and appropriated by living bioplasm of some sort. As the bioplasm of the excreting organs is already surcharged to such an extent as to damage the organs by increase of its bulk, and in other ways, the living matter of the blood, and then that of the tissues of the body, begins to appropriate the excess of pabulum. The white blood-corpuscles increase in size and divide and subdivide, the minute particles of bioplasm grow and increase in number, and the bioplasts of the tissues enlarge, and new centres (nucleoli) make their appearance in them. But these phenomena cannot occur without the action of the tissues being seriously impaired; and, worse than this, the increase of bioplasm in the blood inevitably leads to impeded capillary circulation, and to stagnation of the fluids in the substance of all the tissues of the body—the result of which, if local, must be destruction to the part of the tissue involved; if general, fatal to the whole organism.

But of the two circumstances just spoken of—the decomposition of albuminous matters, and the excessive growth of the bioplasm—the latter is by far the least dangerous, for the first is almost necessarily fatal, and rapidly so, But much of the noxious substances which have accumulated may be removed from the circulating fluid by the growth of the bioplasm, and

temporarily stored in the form of living matter. Time may be gained for the excreting organs to right themselves, and that most favourable symptom in all cases—free excretion—may occur, in which case the surcharged bioplasm becomes at once reduced in volume, and is ready to appropriate more of the dangerous pabulum. The blood is thus relieved. Its bioplasm again takes up the excess of pabulum in the tissues, whose bioplasm gradually returns to its former volume, and there is every prospect of the normal balance of the nutritive and destructive processes being gradually restored. If however—as but too frequently happens—the proportion of the noxious matters already formed be very great, the bioplasm continues to increase unduly; and, as has been mentioned, this increase may lead to a fatal result. But still, the state of things established by the undue growth of bioplasm, unlike that resulting from putrefactive decomposition, is not immediately or *necessarily* fatal; and if by the process life is destroyed, that event is considerably postponed.

3. *Growth of Bioplasm.*—This, as will have been inferred from the remarks under the last heading, is a very serious change, characteristic of the febrile and inflammatory state, and it is that which most frequently leads to a fatal result, and when recovery takes place not unfrequently causes impaired health or chronic disease. The change was referred to in my "Report on the Cattle Plague" published in 1866, and before that time had been fully described in my lectures at King's College. The alterations are most remarkable, and have been figured by me in numerous illustrations. It is this process of undue growth of bioplasm, which from the first we must endeavour to control by treatment. Although it is some years since I pointed out this most important fact, and laid great stress upon the increase of bioplasm in all fevers and inflammations, it has not, I think, yet attracted much attention. The phenomenon has been discussed by me in several memoirs, and in this place I propose to consider the matter mainly in its practical bearing. Not only shall I be able to show that the increase of bioplasm affords a highly important indication for treatment, but that many different remedies which experience has proved to be of use in febrile and inflammatory diseases are valuable, on account of the influence they exert in checking the growth of bioplasm and preventing the impending destruction of tissue; and although in too many instances the process has proceeded beyond our control, yet even in this case we can often retard the inevitable result.

In bad cases of fever and general inflammation the patient may die, as already indicated, from failure of a congenitally weak heart; but the life of those who have the advantage of a

strong and vigorous organ is often destroyed at a later period of the malady by the excessive growth and multiplication of bioplasm in the blood, one consequence of which is plugging of so many capillary vessels of the tissues as to lead to complete suspension of their action, and to damage or destroy their structure. If life be preserved, the structure of the capillaries, nerves, and adjacent textures may be irreparably damaged; and in this way parts of organs of the highest importance to life may be so altered that they can never regain their former healthy condition, in which case the organism will never be so sound, healthy, and vigorous as before.

In the tissues of many cases of death from fever of many different kinds, both in man and animals, I have seen the capillary vessels and small arteries and veins completely obstructed by minute particles of rapidly growing bioplasm, and in not a few instances the minute vessels are dilated in the interval between two *constricted* points, which leads me to conclude that the bioplasm had actually increased in amount by growth long after the circulation had completely stopped. The walls of the vessels are much altered, and in some instances the bioplasts are five times as large as in the normal state, projecting much into the interior of the vessel, and dividing and subdividing freely —or proliferating, as the saying is.

So far, therefore, from there being increased activity of the circulation (as inferred from the rapidity of the heart's action) associated with the hot feverish state—as used to be supposed—the latter is characterised by restricted capillary circulation and a tendency to complete obstruction of so many of the capillary vessels as to damage tissues and organs and to cause death. Now, it is this tendency to the increase of the bioplasm that he who treats fever must endeavour to avert; and one thing which contributes in a most important degree to effect this end is the maintenance of the force of the heart's action, as has been already pointed out. By promoting free circulation, so as to keep the whole mass of the blood constantly moving, and mingling, and changing, not only is the growth of its bioplasm impeded, but the bioplasm already formed is exposed to oxidation and other changes, which lead to its disintegration, to be soon followed by the removal of the products of its decay. Whenever the circulation flags, the increase of the bioplasm is favoured, and the first abnormal augmentation takes place in those organs, such as the liver, spleen, and lymphatic glands, in which the circulation is slowest in the normal state, and where the bioplasm is renewed according to the very moderate demands of the system in health. But besides this advantageous effect, the free action of the circulation renders possible the removal of many noxious materials tending to produce a par-

alysing or poisonous influence upon the nerves distributed to the capillaries, small arteries, and veins of the skin, urinary organs, and bowels, and the nerve centres with which they are connected, upon the integrity of which the condition health is absolutely dependent.

Such, then, so far as I have been able to make out, is the interpretation of the phenomena which may lead to the destruction of life if bad cases of fever are left to pursue their natural course. "Nature," it seems to me, manifests neither a conservative nor reparative action during the course of fever, and death may result long before the period arrives when natural repair becomes possible. But, then, can "Nature" be held reponsible for the development of the feverish state? The natural history of febrile diseases is pretty well known by this time, and as in many cases we are liable to anticipate increase of feverishness, we may place the patient in a more favourable condition to withstand it than "Nature" unaided can achieve, and we may mitigate the force of a blow, although we are powerless to ward it off.

"Expectant Medicine" in severe cases of fever is not justified by the facts known concerning fever, and an expectant attitude will no more save life than it will extinguish fever-poison, effect sanitary improvements, or preserve people in a state of health which will enable them to resist the influence of disease-germs. Expectancy, as a principle, is no more justifiable than is the giving of harmless pilules or coloured water as a practice. In the treatment of real disease, mere passive expectancy means the denial of knowledge, the ignoring of broad facts of observation and experiment, a contempt for the lessons taught by experience, and a disbelief in all that has been handed down to us by those who have observed, and laboured, and thought before we lived. — *Medical Times and Gazette, Dec.* 16 *and* 30, 1871, *pp.* 731, 789.

---

## 7.—ON THE ADMINISTRATION OF STIMULANTS IN FEVER.

By Dr. LIONEL S. BEALE, F.R.S., Physician to King's College Hospital.

*Alcohol.*—The mode of action of alcohol upon the organism during the febrile state is very complex, and before discussing the nature of the modifications in the pathological changes probably effected by it, it is necessary to refer to the great distinction between the two objects for which wine and other stimulants are given during illness. Alcohol is prescribed—1, For the purpose of promoting digestion, improving the appetite, and relieving unpleasant sensations about the stomach; and

2, With the view of directly influencing those most active and serious abnormal changes which are taking place in the blood and in the tissues in all bad forms of fever, which, if they progress beyond a certain degree, will certainly lead to a fatal result.

I propose to defer the consideration of this latter part of the subject until the action of alcohol in moderate doses in the healthy state and in cases of slight fever has been discussed. The forms in which this substance is taken are very numerous, and nothing is more remarkable than the capriciousness exhibited by different stomachs as regards the reception of alcohol. Some persons like, and can take without suffering, any form of alcohol. With others beer and malt liquors agree well—better than wine or spirits. A certain number can even take porter, but not ale, or *vice versâ*. With some dry sherry is the only wine that will agree. Port wine suits others; while not a few prefer, or can only take without suffering from derangement of the digestive organs, certain hocks or clarets, or perry or cider. Brandy or whisky diluted will often agree when every other kind of alcoholic drink fails; but even pure rectified spirit properly diluted will not always be absorbed by the stomach without exciting discomfort and favouring the development of unpleasant gases, with certain organic acids, among which butyric, acetic, and valerianic are found.

No one has yet been able to give any satisfactory explanation of the fact that a little wine will occasion in some stomachs the greatest disturbance. Within a few minutes, not only is the process of digestion stopped, but there is pain, an unpleasant feeling of nausea, not unfrequently accompanied by an actual desire to vomit. In other persons a glass of wine will occasion no inconvenience at the time, but may lead, in the course of from twelve to twenty-four hours, to the development of that unpleasant collection of symptoms which constitutes what is often termed a "bilious attack." Vomiting, purgation, and free diuresis afford relief; but sometimes the disturbance lasts for days, and is not allayed until the stomach has had twenty-four hours' complete rest from work, or until free action of the alimentary canal and all the glands that pour their secretions into it has been promoted by a dose of mercury. It is, after all, not improbable that this most unpleasant action of alcohol indicates a highly sensitive but not unhealthy action of the nerves of the stomach, and that tolerance of wine and spirits is due to a change which has been induced in the finest nerve fibres—in consequence of which their sensitiveness has been impaired. The tolerance of opium, tobacco, and some other poisons is probably to be explained in the same manner. Nor is tissue change limited to the nerves of the stomach; for it is an un-

questionable fact that many of those persons who habitually subject their tissues to the influence of alcohol and tobacco, or both, at an early age, exhibit very distinctly signs of change in many tissues of the body. They look older; and, indeed, physiologically speaking, their tissues are considerably older, and have deteriorated in a much greater degree, than would have been the case if they had not been exposed to the action of alcohol.

It is very remarkable how great a difference, as regards the capacity for the assimilation of alcohol, is observed in the same person when in ordinary good health, and when suffering from even a slight cold. I have observed this many times myself. When in health a very small quantity of wine will disagree, and not unfrequently give rise to a serious disturbance of digestion; but when one feels depressed and miserable from a feverish cold, three or four glasses of wine may be taken within a very short time with benefit, and with a feeling of immediate relief. Persons accustomed to alcohol in one form may take with advantage some other alcoholic fluid during illness.

If at the outset we have any reason to apprehend that an attack of fever is going to be severe, it is very desirable to administer small quantities of alcohol early in the disease. In this way the stomach may be accustomed to the remedy; whereas, if its use is postponed until the patient is very ill, and alcohol required in very large doses, the stomach is often in so highly irritable a state as to reject it. The patient's life may be in jeopardy from this circumstance, or fatal exhaustion alone may actually destroy him.

*Of Giving Alcohol to Young Persons.*—My conclusions as regards giving alcohol to the young are in the main not at variance with the opinions of those who advocate extreme temperance. My own experience leads me to believe that the majority of young healthy people would do well without alcohol; and I believe the habitual daily consumption by young persons —even of a moderate quantity—of wine or beer, is quite unnecessary, and mere waste, while in some instances it is positively injurious to health. At the same time, there can be no doubt that in certain cases where the health fails in children, and even in infants, great benefit results from giving small quantities of wine daily for a short time. Hard-working people, students, professional men, and people actively engaged have been advised to take stimulants, as a general rule—and some, no doubt, require them; but I believe many would enjoy very good health without any alcohol at all, while the recommendation that they should take plenty of claret or other light wine is bad advice for several reasons. Not only is a bottle of light wine not required, but in many cases it is actually injurious.

That people who can get it will often take a bottle of light wine, and more, is quite certain; but that they require it, or that it is good for their health, will not bear discussion.

Up to the age of 40 very little stimulants is, as a general rule, really desirable for healthy persons, and I expect most people of average health would get on better without any. My own personal experience is this:—I was never very strong, though always able to get through a very considerable amount of physical exertion without suffering from fatigue. Up to the age of 40 I hardly ever touched stimulants of any kind, and when I did take a little I not unfrequently experienced an attack of sick headache before my ordinary condition of health was restored. Lately, however, I have found the advantage of half a tumbler of ale daily; and I can bear half an ounce, and sometimes three or four ounces, of wine without suffering. I dare say, as I grow older, I may, like most persons, require a little more; but when in the country, and taking plenty of exercise, I feel very well and contented without any stimulants whatever. The experience of some members of my family who have lived to be old, and that of many persons of whom I have inquired, accords with my own. In old age, I believe, stimulants are really necessary, and sometimes are even more important than food itself. I feel sure the life of many old people is prolonged by the judicious use of alcohol, and I think that some, who have been very careful all through life, take far too little stimulant when they grow old.

*Of the Probable Action of Alcohol in the Body.*—But we may now very briefly consider the influence of alcohol upon the organism, and its probable operation as an article of diet. What becomes of alcohol when it is taken into the stomach? There is no doubt that if the spirit is strong when introduced, it is much diluted by the pouring out of fluid from the vessels and glands of the stomach, and that it is quickly absorbed, in its diluted state, into the blood. That this is so is proved by the familiar fact that the smell of alcohol is often very perceptible in the breath. Moreover, as is well known, alcohol has been detected by chemical tests in the breath, in the sweat, in the urine, and in the other secretions by a number of observers. Alcohol has also been proved to exist in the blood. There is, therefore, no doubt that alcohol, as alcohol, may not only be taken up by the blood, but may circulate with the nutrient fluid, and eventually pass away from it unchanged. But it must not therefore be concluded that *all the alcohol every person takes* is thus absorbed as alcohol, caused to circulate through the body as alcohol, and at last excreted unchanged; for such a conclusion would be opposed to the facts of observation and experiment. The truth seems to be, that some of the alcohol

taken is unchanged in the system, but that a considerable and very varying proportion of the total quantity introduced is caused to disappear altogether as alcohol, and to pass through most important changes, escaping at last from the organism probably as carbonic acid and water.

A certain quantity of alcohol is *digested* and *assimilated;* and it is quite certain that the capacity for the digestion of alcohol varies very remarkably in different individuals. It is most probable that the alcohol is taken up by, and carried with, the portal blood to the liver. It is then appropriated with other substances by the bioplasm of the hepatic cells, and completely changed. Its elements are rearranged, and added to the constituents which form the liver-cell, and which gradually break up to form the ingredients of bile, the liver-sugar, and the so-called amyloid matter.

It is the living matter of the yeast-cell that splits up to form alcohol and carbonic acid, water, and a form of cellulose. We shall not be surprised to find that another form of living matter—that of the liver-cell—has the power of appropriating alcohol, rearranging its elements, and causing them to combine with other elements to form compounds having properties very different from those of the materials out of which they were made. And it seems probable that under certain circumstances other forms of bioplasm of the body are able to take up and appropriate alcohol; for it is certain that in some prolonged cases of exhausting disease a large amount of alcohol is readily assimilated, while ordinary foods can only be taken in such infinitesimal amount that we cannot attribute to them much influence in the maintenance of life. In severe cases of fever, as I shall again have occasion to state, the greater proportion of the alcohol introduced is probably not oxidised, as used to be supposed, but appropriated. Its effect is to lower, not to elevate, the temperature; and, so far from increasing the dyspnœa in bad cases of bronchitis, pneumonia, etc., by throwing increased work upon the lungs, as used to be affirmed, it has a directly contrary effect.

Dr. Parkes has shown that diluted alcohol, given daily in such proportions that not more than two ounces of absolute alcohol are consumed in the twenty-four hours, in most cases improves the appetite, and slightly quickens the heart's action; but that larger amounts have an opposite effect as regards the appetite, and greatly increase the cardiac beats.

Anstie and Dupré showed that if doses of alcohol sufficiently large to produce narcotic effects are taken, alcohol escapes in the excretions, but when smaller quantities are taken it is not to be detected. This may be the true explanation of the fact that alcohol in certain cases cannot be detected in any of the

secretions at all. It is certain that the quantity required to produce narcosis varies greatly in different individuals, and perhaps this may account for the different results obtained in the course of different experiments.

Dr. Dupré has quite recently proved that, of the alcohol taken in moderate doses (48 to 68 grammes of absolute alcohol), only a minute fraction is excreted as alcohol, while by far the larger proportion is disposed of in the system in some other manner. Dupré's observations show that this alcohol is not stored up in the system as alcohol, and slowly evolved in the form of alcohol. He remarks that the amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet, and that, therefore, all the alcohol taken daily must be disposed of daily, and converted into some other substance in the system.

We must therefore conclude that, of the alcohol taken, only a small but very variable amount is excreted as alcohol, but that the larger proportion, at least in the case of most organisms, is changed in the system; not simply acted upon by other things in a state of change, as may be effected out of the body, but actually taken up by the living matter or bioplasm, appropriated and converted into other substances. Though probably not applied to the nutrition of tissues, its elements may perhaps assist to form some of the constituents of bile, sugar, fatty, and amyloid matter.—*Med. Times and Gazette, Mar.* 2, 1872, *p.* 245.

---

## 8.—THE BOWEL LESION OF TYPHOID FEVER; ITS NATURE AND TREATMENT.

By Dr. T. J. Maclagan, Dundee.

[Dr. Maclagan's views on the bowel lesion of typhoid fever are considerably at variance with those usually entertained on that subject.]

This lesion consists in inflammation and ulceration of the agminated and solitary glands of the small intestine. These glands are scattered over the last few feet of that portion of the bowel, increasing in number as we pass downwards, until at the extremity of the ileum they are so numerous and closely aggregated together that but a small part of the mucous membrane remains free from them. They are imbedded in the submucous tissue and covered over by the mucous membrane; they have no external opening, but are blind, ductless glands. The solitary ones are also found sparsely distributed over the large intestine, diminishing in number the further we pass down from the cæcum.

The nature and function of these glands in a state of health have been matter of some difference of opinion; some have supposed that they are secreting cells, which periodically burst

and discharge their contents into the intestine; others regard them as absorbent glands, which have no share in the production of the intestinal contents, but are somehow engaged in the elaboration of the fluid which is ultimately carried away by the lacteals. The latter view is the more generally adopted, and is, no doubt, the correct one. The appearance which these glands present under the microscope is very similar to that presented by the medullary cords of the lymphatic glands; and Brucke succeeded in injecting them through the lacteals—a proof of their connection therewith. Their position in the intestine also supports this view; for if they were secreting cells, and if their function consisted in supplying to the intestine something requisite for the digestion of its contents, they would be situated high up in the gut (as are Brunner's glands and the bile and pancreatic ducts), where their secretions would be brought into play and utilised at an early period of the process of digestion. The sphincter ilei, or band of muscular fibres at the extremity of the ileum, probably has for its function the narrowing or temporary occlusion of the outlet of the ileum, and consequent detension of the chyle in contact with these glands.

The bowel lesion of typhoid fever consists in inflammation, sloughing, and ulceration of these agminated and solitary glands. What is the connection between this local lesion and the febrile symptoms? Is the constitutional disturbance a mere symptom of the bowel disease, or is this a result of the circulation in the blood of the poison of the malady?

The generally entertained opinion is that the bowel lesion is the result of nature's efforts to eliminate the poison from the system; that the deposit which takes place in and around the glands is specific in character; and that its deposition in and elimination by these glands is the means of ridding the system of the poison which produced the febrile symptoms. The sole facts in favour of this view are—(1) that inflammation of these glands is an invariable accompaniment of typhoid fever, and (2) that the discharges from the bowels largely contain the poison of that disease.

Such a view of the nature of this important lesion I believe to be altogether erroneous, and that for the following reasons:—

1. The glands are possessed of absorbent functions only, and are in no sense eliminatory.

2. Even if they did possess eliminatory functions, it is very improbable that the work of elimination would be thrown to so large an extent on the glands at the lower extremity of the ileum, while those high up escaped altogether.

3. Were they eliminatory, they would not probably be des-

troyed (as they are in typhoid fever) while performing a duty so necessary to the well-being of the body.

4. Were the elimination of the poison by these glands nature's way of relieving the system, the deposition in them of the morbid material ought to be followed by an amelioration of the general symptoms of the disease. We know that the contrary holds good, and that the bowel lesion increases rather than diminishes the general febrile disturbance.

5. The opinion that the poison is thus eliminated is at variance with the fact that mild cases of typhoid fever do occur in which the glandular lesion terminates in resolution, and in which the so-called morbid deposit is reabsorbed into the system.

For the above reasons I reject the views generally entertained regarding the nature of this lesion, and hold that it is a specific inflammation of the agminated and solitary glands consequent on the action of the poison of typhoid fever on these glands, and bearing to that disease the same kind of relationship that inflammation of the tonsils does to scarlatina, and inflammation of the mucous membrane of the respiratory passages to measles.

I believe also that the sloughs and discharges from the ulcerated glands carry the poison of typhoid fever, and are capable of conveying that disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina and measles are capable of transmitting their peculiar poison. Nay, I go a step farther, and maintain that the contagious nature of these discharges is not done full justice to by a simple recognition of their power to convey typhoid fever from one person to another, but that *their propensity to evil is coincident with their existence, and that they can, and often do, impart the specific inflammation of which they are the product to the unaffected glands with which they come in contact in their course down the intestine.* I hold, in short, that in typhoid fever there are two forms of bowel lesion, a *primary* and a *secondary:* the primary lesions commence coincidently with the onset of the febrile symptoms; the secondary result from the inoculation of healthy glands by the discharges from those primarily involved.

The grounds of this belief are various, and may be briefly stated as follows:—

1. It is consistent with all pathological analogy that the discharge from a specifically inflamed organ applied to a like organ in a state of health should there reproduce that specific inflammation to which it owes its origin.

2. Examination of the individual lesions shows that they are of two kinds—one in which the deposit is scanty and the mucous membrane granular in aspect, and another in which

the deposit is more abundant and the mucous membrane smooth: to the former Louis gave the name of *plaques molles*, to the latter that of *plaques dures*. Besides this difference in appearance and in amount of deposit, there is also a difference in the mode in which they ulcerate. In the *plaques dures* the process commences in the interior, and the mass sloughs out bodily; in the *plaques molles* the process of destruction commences by a superficial ulceration which eats its way inwards through the mucous and submucous coats, and may even penetrate the muscular and peritoneal. No explanation of this difference has ever been given. The real distinction between them I believe to be, that the *plaques dures* are the primary lesions, the *plaques molles* the secondary. The explanation of the difference in their external characters is, that in the primary lesions (*plaques dures*) the inflammatory product is slowly deposited in the glands and surrounding submucous tissue, and continues to be deposited for eight or ten days before sloughing takes place; while in the secondary lesions (*plaques molles*) the direct application of the virus and putrid discharges sets agoing a smart inflammatory action which terminates in ulceration before there is time for the deposition of sufficient inflammatory products to give them the external appearance of the primary lesions.

3. The glands of the lower part of the ileum suffer more than those higher up. When considering the normal physiological action of these glands, it was noted that they became more numerous the further down the ileum we went. This peculiar distribution has for its object to secure that no waste takes place; while the contraction of the sphincter ilei muscle retains the chyle in the lower part of the small intestine, the presence of so many absorbent glands in that locality provides against the passage into the large intestine of any quantity of the nutrient material which ought to be taken up in the small. The contents of the ileum in a state of health consist of such nutritious ingredients of the chyle as have not yet been absorbed, epithelium, and the excretory matter which goes to form the fæces in the large intestine. In typhoid fever they consist of the same materials *plus* the sloughs and discharges from the affected glands situated higher up. In a state of health the numerous glands in the lower part of the ileum, in the exercise of their absorbent functions, take up the nutrient material remaining in the chyle, and the rest of the iliac contents pass into the colon. In typhoid fever they take up the same material, but along with it they absorb the virulent discharges proceeding from the affected glands situated higher up. The discharges from the secondary lesions are as potent for evil as those from the primary. Hence it is that the lower part of the

ileum suffers so much more than the upper; for the further down the small intestine we go below the highest affected gland, the greater is the amount of virus in a given quantity of the contents of the bowel, and the less the likelihood of any gland escaping. On careful examination of the diseased intestine after death, it is found that "the transition between the diseased and healthy glands is usually rather abrupt, and proceeding downwards after the first diseased patch all are usually diseased" (Murchison). This fact can be satisfactorily accounted for in no other way than that which I have indicated. The greater abundance of these glands at the lower extremity of the ileum doubtless leads to a correspondingly greater frequency of primary lesions in that locality; but for the explanation of the fact that a healthy patch is rarely, if ever, found below a diseased one, it seems to me absolutely necessary to recognise the influence of an agency which acts from above downwards, and which comes into play at the upper margin of the disease. Such an agency can be no other than the sloughs and discharges from the affected glands, which, in their passage down the gut, tend to inoculate those below, but cannot affect those above the highest point at which they are formed. An apparent objection to this view, that the ulcers near the ileo-cæcal valve are as far advanced as those higher up, is explained away by the fact that the secondary lesions ulcerate much more rapidly than the primary, and are in a week as far advanced in their pathological course as those primarily affected a fortnight earlier.

4. The extent of the intestinal lesion, as revealed by examination after death, bears no relation to the severity of the abdominal symptoms during life. The severity of the abdominal symptoms is judged of very much by the amount of diarrhœa. In cases in which the bowels are moved several times a day the sloughs and discharges are carried off as quickly as they separate, and thus do their minimum of harm. Where constipation exists they are retained, and so have an increased chance of inoculating healthy glands. Where constipation exists there may thus be produced an amount of local disease greater than would have existed had the abdominal symptoms been more marked, and the discharges carried off more speedily.

When considering the difference between the primary and secondary lesions it was noted, *inter alia*, that in the primary lesions the process of destruction commenced in the interior, the diseased gland forming a slough which separated bodily, while in the secondary the destructive process commenced on the surface, and proceeded inwards. As a consequence of this mode of formation of the ulcers, the primary lesions seldom extend beyond the base of the glands—that is to say, they

involve the mucous and submucous coats, reaching, but not penetrating, the muscular. In the secondary lesions the ulcerative process commences on the surface, and proceeds inwards, rapidly destroying the mucous and submucous coats, but does not have the same reason for stopping there, and hence is more apt to invade the muscular and peritoneal coats. In this way is to be explained the fact that perforation most frequently occurs in cases in which, up to the moment of its occurrence, the abdominal symptoms have been by no means marked. This accident, indeed, often happens in cases in which such symptoms have been altogether wanting. How is this? It seems very like a paradox to say that perforation, the acme of all that is severe in the bowel lesion, is most common in cases in which the evidence of that lesion is almost or altogether wanting. Paradoxical as it appears, it is nevertheless true. The explanation of this fact is that the perforation is the result, not of a primary, but of a secondary lesion; for if in these mild cases the number of glands primarily involved is comparatively small (as I believe it is) there remains a larger number of sound glands liable to inoculation; and as the risk of perforation is directly as the extent and number of the secondary lesions, it follows that the probability of that accident occurring as a consequence of such a lesion bears some relation to the mildness of the attack. It is to be noted, too, that perforation generally occurs low down in the ileum, where we have seen that secondary ulceration is most abundant and most active.

Hemorrhage, too, when it occurs to any extent, usually proceeds from a secondary ulcer. So long as the mucous and submucous coats only are involved profuse hemorrhage is not likely to occur, there being in them no vessels sufficiently large for the production of such a result. When the muscular coat is invaded, however, the risk is much increased, for between the transverse and longitudinal fibres there run vessels of considerably larger size than those found in the submucous tissue. These vessels run a great risk of being opened into by an ulcer which eats into the muscular coat. This it is which imparts to even a slight hemorrhage occurring late in the case an importance which would not attach to it at an earlier period. It indicates a still progressive secondary ulcer, and there is no saying where such an ulcer may stop. It may open into other vessels, or involve the peritoneum in its destructive course.

The time when the sloughs are separating is generally, I had almost said universally, regarded as the period of greatest danger, so far as the bowel lesion is concerned. Experience shows that the danger from that lesion increases the longer the fever continues, and that from the beginning of the fourth week

onwards it is at its greatest. Now it can scarcely be supposed that a slough, which is formed during the second week (as most if not all of them are), which is necessarily of limited size, and which is seated in a very vascular structure, should take a fortnight or three weeks to separate. Yet such a conclusion is the inevitable outcome of the generally entertained opinion. The position seems to me quite untenable. I believe that the whole process of inflammation and destruction of the glands primarily involved, and separation of the resulting sloughs, never occupies more than three weeks, and that the prolongation of the disease beyond that period, and all the consequent risks and dangers, are attributable to the formation of secondary lesions, the distinguishing characteristics of which are the rapidity with which they are formed and the greater tendency which they show to extend beyond the submucous coat. I believe that hemorrhage seldom, if ever, occurs before the primary sloughs begin to separate, and that the bleeding which occasionally takes place at that time is rarely a source of danger. It is generally very slight. Dangerous hemorrhage, as already explained, comes on late in the case, when the primary sloughs are detached, and when the secondary ulcers are still pursuing their destructive course, threatening the larger vessels of the muscular coat.

In the vast majority of cases in which death is attributable to the intestinal complication, the fatal event is the result of the destructive ulceration of the secondary lesions.

When a patient dies of typhoid fever within the first fortnight, death is due to the intensity of the poison and not to the abdominal mischief. In most cases in which the bowel lesion is the source of danger, the urgent symptoms are developed late in the case—seldom before the middle or end of the third week, generally during the fourth or fifth, by which time it may be presumed that the sloughs have separated from the primary lesions; the exacerbation of the local mischief being due to inoculation of fresh glands by the poisonous discharges of those primarily affected.

If the discharges are so virulent, why, it may be asked, do they not affect the solitary glands of the large intestine? In the majority of cases these glands do escape. This is attributable chiefly to their small size and sparse distribution, it being evident that the risk of a drop of virus bearing discharge lodging over a small solitary gland in the capacious large intestine is very much less than the chance of its lodging over a good-sized patch in the attenuated small intestine; in the latter, too, the discharges are retained for a time in contact with the glands by the action of the sphincter ilei, while in the former they are in constant motion.

The successive inoculation of individual glands by the discharges from those already affected offers a ready explanation of the protracted duration of many cases of typhoid fever. The simultaneous inoculation of a number of glands accounts for those sudden and often alarming exacerbations of the local and general symptoms which so frequently convert an apparently mild into a severe attack. No matter whether the aggravation of the febrile symptoms takes place before the cessation of the primary fever, or some days afterwards (constituting a true relapse), it is due to one and the same cause, inoculation of healthy glands by the discharges from those already affected. The alarming symptoms which are often developed in the fourth and fifth weeks of typhoid fever are the result either of the absorption by glands which had hitherto escaped of the putrid discharges coming from above them, or of the destructive ulceration of the secondary lesions. Occurring while the primary fever still exists, such inoculation gives rise to more alarming symptoms than are observed in a true relapse, in which the second seizure is generally mild, probably in consequence of the diminished scope for intestinal mischief resulting from the destruction of so many glands during the primary seizure.

The age and mode of life of the patient have a material influence on the severity and course of typhoid fever.

*Age.*—As already explained, typhoid fever increases in frequency and severity from the earliest period at which it occurs up to the time at which the vigour and elasticity of the frame are greatest; when that period is passed it begins to descend, and becomes less and less common as years advance, till we find that after fifty it is a rare and not very fatal disease. In this respect it presents a marked contrast to typhus, which so increases in severity and fatality with advancing years that recovery is far from common after fifty, and is scarcely to be looked for at sixty and seventy. The period of life during which typhoid is most prevalent and fatal is that during which the intestinal glands which are specifically affected in that disease are most prominent and active, and that, in its turn, may be said to be the period of activity of the reproductive organs. Typhoid fever is therefore most fatal in early manhood.

*The mode of life of the patient* has a very important influence on the severity of the attack. All who have seen much of the disease in both hospital and private practice will agree with me that it is more fatal among the better classes than among the poor. Why is this? Hospital patients are not so well nourished, are often the victims of considerable privations, and seldom come under notice till they have been ill a week or ten days; one would therefore be disposed to think that they would be

less able to bear up against the disease. I have seen it stated (I do not remember where) that people in the higher ranks do not bear up against such a malady so well as those in a humbler sphere because, being in the daily habit of using a more generous diet and a considerable quantity of wine and other stimulant, they do not so readily respond to stimulants when these become necessary to counteract the depressing influence of the disease. Such an impression I believe to be quite erroneous. A drunkard of course suffers more severely from fever or any other ailment than a temperate man, and is less easily kept up; but such patients are more common in hospital than in private practice. The greater fatality of typhoid fever among the better classes is due to quite another cause. It is due, not to any difficulty in getting them to respond to the action of stimulants, but to a really greater severity of the disease; the malady takes a greater hold of them, and they consequently suffer more from it, independently of all treatment.

We have seen that the glands which suffer in typhoid fever are absorbent, and that their function probably consists in taking up from the chyle something which the system requires for the maintenance of the vigour and elasticity of youth. Now it may be taken for granted that the richer and more abundant the chyle the greater will be the work which these glands have to do. Of course the chyle will be richest in those who live best (*cœteris paribus*), and hence it must happen that the absorbent glands of the better classes of society will be more actively and constantly employed than the same glands in the poorer classes.

It may be regarded as a physiological law that an organ's capacity for work increases with the demands made upon it: frequent exercise increases the size and strength of the muscles; obstruction to the flow of blood from the heart produces hypertrophy of that organ; stricture of the urethra produces thickening of the muscular coat of the bladder; when one lung is in abeyance the other does increased work; cessation of the function of one kidney results in increase in the size and efficiency of the other. So it is with these glands; the richer chyle which passes along the alimentary canal of a man who constantly uses a highly nourishing and stimulating diet supplies more of that substance which it is their function to absorb than does the chyle of a man whose diet is sparing. The continued use of such a diet for a length of time must lead to increase in the size and activity of these glands. The condition thus produced is much more favourable to the development of the poison of typhoid fever than that which obtains in the poorly-fed hospital patient. Should this poison be received into the system of one whose glands are so highly developed there

inevitably results a severer attack of the fever than the same poison would produce in one whose glands were smaller and less active. The greater fatality, then, of typhoid fever in the higher ranks of life is the result, not of any difficulties in the way of treatment, but simply of the greater severity of the disease consequent on the more highly developed state of the intestinal glands and the necessarily increased facilities afforded for the growth and reproduction of the contagium in the system, as well as the exaggerated dangers of the local mischief.

Chomel and Forget have both expressed the opinion that debility from destitution is a favourable circumstance as regards prognosis in typhoid fever. According to the views generally entertained regarding the nature of that fever, this statement is little better than a paradox; to say that debility from destitution positively favours recovery from a disease which kills by asthenia is to make a statement which requires the high sanction of such eminent names to prevent us receiving it with scepticism, if not actual incredulity. The explanation just given of the greater fatality of the disease in the upper ranks of society supplies also a reason for its being less fatal among the poor and destitute, and affords evidence of the accuracy of the observation made by Chomel and Forget.

*Treatment.*—It may readily be supposed that these peculiar views regarding the bowel lesion of typhoid fever have some influence on the treatment which I adopt. They do influence it very materially.

The opinion generally entertained by medical men in this country regarding the treatment of typhoid fever is that it consists in keeping up the strength of the patient, in procuring sleep, and in checking every tendency to diarrhœa. Regarding the desirability of keeping up the strength and of procuring sleep there can be no doubt. To the general treatment I shall not further refer. I wish only to direct attention to the treatment of the bowel lesion. There are two methods of treating this. One is that recommended by Bretonneau and Louis, and adopted by most French physicians: it consists in encouraging rather than checking the action of the bowels. The other is almost universally adopted in this country, and consists in checking every tendency to diarrhœa. Dr. Todd thus summed up the treatment:—"Restrain diarrhœa and hemorrhage in typhoid fever; and when you have fairly locked up the bowels, keep them so. Patients will go for four or six days, or even longer, without suffering inconvenience from this state of constipation." Such is the treatment adopted in ninety-nine per cent. of the cases of typhoid fever which occur in this country. Dr. Todd's opinion is that of the profession in general.

The treatment proper for the bowel lesion of typhoid fever

varies in different cases, and in the same case at different times. To know what is best for the patient, we must know the probable condition of that portion of the bowel which is the seat of disease. The general opinion is, that inflammation and irritation of the mucous membrane, by causing an increased flow of the secretions, produce a tendency to diarrhœa which it is well to check, and if possible prevent. Granting the soundness of the pathology, the treatment is right enough. I hold, however, that the pathology is unsound, and that the practice of "locking up" the bowels is one which leads to a prolongation of the disease, and increases the risk to the patient.

If all the lesions were primary, and if there were no chance of the secondary inoculation of other glands, constipation might be regarded as a favourable sign. The recognition of the occurrence of secondary lesions alters the whole question, and is the true pathological foundation of the treatment which I have to recommend; it supplies, too, a more satisfactory reason than has hitherto been advanced for the success which has attended the French practice.

In the early stages of typhoid fever it must ever be borne in mind that in many cases resolution of the intestinal lesion is possible, and that judicious management during the first ten days may cut short and render comparatively trivial an ailment which the patient's own indiscretion, or injudicious treatment on the part of the physician, may convert into a long and dangerous illness. So long as such resolution is possible, the patient must be treated for that result. He must be confined to bed, and absolute rest and quiet enjoined. It must be borne in mind that some of the glands of the small intestine are inflamed, and consequently more than usually prominent and tender. Under these circumstances, every possible source of irritation to them must be avoided: for this reason no article of diet should be taken which contains much indigestible matter; for the contact of this with the glands on its passage down the gut would only serve to irritate them more. *Physiological rest must also be obtained as far as possible.* For this reason too much nourishment should not be given. The object in view is rest to the affected glands: to attain this object we must give only such articles and such quantities of food as will be digested and utilised in the upper part of the alimentary canal, leaving little or nothing to be done by the inflamed glands. The diet therefore should be mainly composed of milk, given, not *ad libitum*, but in stated quantities and at stated times.

The bowels should be kept as nearly as possible in their natural state. Any tendency to diarrhœa should be counteracted by the addition of lime-water to the milk, and frequently repeated small doses of Dover's powder. Constipation (which

is more common in mild cases) is to be obviated by some gentle laxative. I prefer the phosphate of soda to any other; it is not at all irritating, acts very gently, and, having very little taste, is readily taken dissolved in a little warm milk or gruel: from half a drachm to two drachms may be given once or twice a day as required. At this stage of the illness ipecacuanha is very useful: given in small doses—one fourth to one-sixth of a grain—every two hours, it acts beneficially not only on the skin, but on the mucous membrane of the bowel. When there is much heat of skin it is best given in the form of wine combined with liquor ammoniæ acetatis. In administering laxatives, it is best to give those which act on the upper part of the alimentary canal.

Such are the means calculated to favour resolution of the bowel lesion.

In all cases after the disease has continued for a fortnight without signs of improvement, and in severe cases at an earlier period of the illness, the hope of attaining such a favourable result may be abandoned. Treatment must now be directed, not to the prevention of sloughing of the glands already involved, but to the prevention, as far as possible, of secondary lesions by the inoculation of glands which had hitherto escaped.

It must be remembered that we have to deal with an intestine in which there are a number of sloughing glands and a number of healthy ones. The liability of the latter to inoculation by discharges from the former evidently depends upon the relative position of the two sets; the higher up the bowel the inflamed glands are situate the greater will be the risk of secondary lesions. It is impossible to tell the exact position of the affected glands. We must act on the supposition, I had almost said the certainty, that there are some glands exposed to inoculation, and do what we can to save them. How is this to be done? Certainly not by shutting up in the intestine the noxious sloughs and discharges. For what is taking place during this quiescent state of the bowels? The sloughs and discharges are being constantly separated from the primary lesions, and the healthy glands situated further down are being exposed to the influence of a poison which has a specific action on them, and which is hourly increasing in strength and quantity the longer constipation exists. By encouraging and keeping up this condition of the bowels we positively increase what we fancy we are guarding against—extension of the bowel lesion. To prevent altogether the formation of secondary lesions is impossible, but it is evident that no means which tend to keep the discharges from the primary lesions in the neighbourhood of healthy glands can have other than a deleterious effect, and

that any means calculated to ward off this danger must have for its object the carrying off of the sloughs and discharges as soon as they are separated. Hence the objection to the practice advocated by Todd, and acted up to by most physicians in this country; and hence the reason for adopting the opposite line of treatment. After the inflammation of the primary lesions has gone on to sloughing, the patient's welfare demands that the sloughs should be got rid of as soon as possible; this end can only be attained by keeping the bowels open. If constipation exists it should be overcome by small doses of castor oil, phosphate of soda, or other laxative. Looseness should not be checked unless the stools are so frequent or copious as to affect injuriously the patient's strength. It may be stated generally that so long as there are not more than three stools a day astringents should be withheld. The diarrhœa is not entirely due to the actual lesion of the bowel, but is in part due to the irritation caused by the fetid discharges, which it tends to carry off. It is therefore salutary so long as it is not so severe as to prostrate the sufferer.

Space forbids me to enter on the consideration of the question whether or not the typhoid symptoms of enteric fever are at all due to the absorption of pus or putrid discharges from the bowel lesions. I would only remark that, while they are not due to that cause in every case, it is quite possible for such an accident to occur, and that the recognition of such a possibility is an additional reason for preventing their undue retention within the bowel.

When diarrhœa is excessive it must be restrained, not stopped. For this purpose, acetate of lead, dilute sulphuric acid, liquor ferri pernitratis, or other astringent, with or without opium, may be administered. The liquor ferri pernitratis is particularly serviceable; it is possible that some of its beneficial action may be due to a corrective action on the fetid alkaline contents of the bowel.

The opinions which I have expressed are at variance with those generally entertained; but they are the result of years of careful study of the subject, and of a large experience in the treatment of typhoid fever.—*Lancet, Jan.* 20 *and* 27, 1872, *pp.* 75, 110.

---

## 9.—THE TREATMENT OF HYPERPYREXIA BY WITHDRAWAL OF HEAT.

By the Editor of the British Medical Journal.

The careful study of the temperature in disease within the last twenty years has more and more led to the conclusion, that in all febrile diseases the increased blood-heat is in itself

one of the greatest dangers to life; and this conviction has been the predominating idea in all the recent attempts at a more effective treatment of fever. As the revival of the use of the thermometer took place in Germany, so has also the practical application of the principles derived from its indications chiefly been worked out in that country. The school of the late Niemeyer stands foremost in the ranks of workers in that field; but to E. Brand of Stettin, who first treated a considerable number of typhoid patients on the hydrotherapic plan, and to the enthusiastic proclamation of his success, is mainly due the impulse to the zeal with which the subject has been taken up in the clinical hospitals all over Germany, and by which, at last, the cooling treatment of the pyrexial state, by the external application of cold, has been placed on a thoroughly scientific basis. Not only in most of the German civil and military hospitals is this method of treatment now the general rule; it has also, to a great extent, found its way into private practice. The statistics published by Jürgensen, by Liebermeister and Hazenbach, by Ziemssen and Immermann, and by many others, comprising over a thousand cases of typhoid fever, treated in various hospitals in the north and south of Germany, show that the mortality has, under this plan of treatment, been less than half of what it had been previously, varying now only from three to ten per cent.

With such results before us we ought no longer to stand by as passive spectators, surrounded as we constantly are by various forms of continued fever. Typhus being of rare occurrence on the Continent, the observations on the effects of the cooling treatment in that disease are as yet not very numerous; but where it has been tried, the results were no less encouraging. There would be ample opportunities in the fever hospitals of this country to supply more extensive experience in this disease; and we would urge upon the managers of our various fever hospitals the great importance of giving this method of treatment a careful trial, on a large scale, in typhus as well as in typhoid fever. There might be further scope for its employment in scarlet-fever and small-pox, not to speak of cases of high symptomatic pyrexia, as in pneumonia, in which baths have recently been employed by Lebert and others.

[The treatment of fevers by the withdrawal of heat is nothing new in this country; it was practised by Dr. T. Currie, of Liverpool, at the end of the last century, with great success.]

In contagious fever, such as typhus or scarlatina, in which we may assume that the specific poison is thrown out on the skin, the effect of baths in diminishing the danger of infection must not be overlooked—a point to which Currie has already

called attention. We must always bear in mind that this method of treatment is not directed against the disease itself, but simply against a symptom of the disease, from which a series of other symptoms depend. No means having as yet been discovered to neutralise any of the supposed poisons of the specific fevers after they have once begun their work in the system, we must content ourselves with counteracting, as much as possible, their effects, not only by carefully feeding the patient, but also by removing, as much as lies in our power, the chief symptom, which undoubtedly exerts a pernicious influence on the most vital tissues—namely, the febrile heat of the body. It is not, therefore, by some mysterious operation that, as some water-doctors in old and modern times would make us believe, the treatment by baths and other "hydropathic" measures acts. The effect of these proceedings is, it need hardly be said, a purely physical one, which can be analysed just as any other physical phenomenon. The subject of the animal heat in health and disease, and under various external influences, is, no doubt, a most complicated and difficult one to deal with; yet, although some important points are still undecided. it has, thanks especially to the labours of Liebermeister, been sufficiently cleared up for all practical purposes. The effect of a single cooling proceeding on a patient with high pyrexia has been found to be this. In the first few minutes of the external application of cold, the internal temperature, as measured in the rectum or axilla, rises a few tenths of a degree; then a more or less rapid decline of the mercury takes place, as the heat is being rapidly withdrawn from the skin and subjacent tissues. This diminution of the internal temperature does not stop with the termination of the application of cold, but continues for several hours afterwards, the blood giving up heat to the cooled surface of the body; and the difference between internal and peripheral temperature, at first great, becoming gradually more and more equalised. Then the temperature rises again, and the previous, or even a higher, degree of heat may be reached after some time. The degree of cooling in and after a bath—*i.e.*, the effect of the bath, which may amount to from two to eight degrees F., depends upon the temperature and duration of the bath, the temperature and size of the patient, the period of the disease and the time of the day—*i.e.*, the period of natural fluctuation of the temperature at which it is given. Practically, the principle of this treatment lies in a nutshell. We know that the fever-heat is a most dangerous agent; there exist certain means to keep it down, and we have evidence that these means prove eminently successful in practice. To keep the average daily temperature below a certain range—say below 100°—must, therefore, be the aim of our treatment. It is quite

clear that such a treatment cannot be carried out safely without the constant use of the thermometer. The temperature of the patient must be observed every two or three hours during the day and night; and whenever a certain degree is reached, which according to the circumstances of the case may be fixed at 103°, or 103·5° in the axilla (103·5 or 104° in the rectum), we must interfere. The number of baths or other cooling measures which thus becomes necessary varies according to the severity of the fever, the efficiency of the proceeding, and the stage of the disease. In several of the recorded cases of typhoid fever, as many as twelve baths in the twenty-four hours have been necessary in order to keep down the temperature. In milder cases, four baths daily were sufficient. As the fever declines, the daily number of baths becomes less.

The only contraindications to this treatment are, first of all, hemorrhage from the bowels in typhoid fever, even if it be present in the slightest degree; perforation also precludes its continuance, for which, moreover, there would hardly ever be any indication, as peritonitis following perforation, even if life last, is generally accompanied by collapse and low temperature. Should it be high, ice-bags on the abdomen may be used. Some rare nervous symptoms, such as epilepsy or paralysis, may also forbid the use of the more powerful means of cooling the body. Menstruation, on the contrary, has not been found to necessitate the interruption of the treatment; nor does pneumonia or bronchitis contraindicate it.

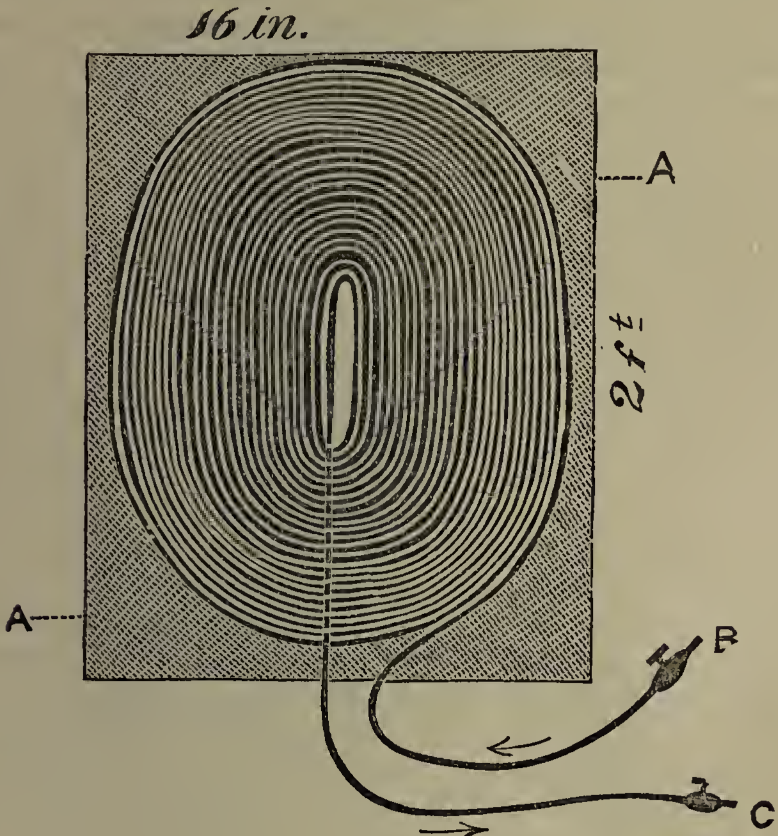
Various methods have been used for the purpose of cooling the body; and it is of great importance to know their relative value. Liebermeister, who has experimentally approached this difficult question, ranges them in the following order; cold affusion, cold wet sheet packing, cold bath. The cold affusion, as practised by Currie, has much less effect in abstracting heat than a full bath of equal temperature and duration. With regard to the cold wet sheet, Liebermeister has found that a series of four cold packings, one following upon the other as soon as the sheet becomes warm, has about the same effect as a cold bath of ten minutes' duration, and double the effect of the cold affusion, Although some patients are fond of, and even ask for, cold baths, yet it cannot be denied that the majority of them have a dislike to sudden great differences of the surrounding temperature. This objection has been successfully removed by Ziemssen, who recommends to begin the bath with a temperature only about ten degrees lower than that of the patient, and then gradually, by adding cold water, to reduce the temperature of the bath to about 68°. Such baths are extremely grateful to the patients, who remain in them twenty-five to thirty minutes. By these various methods ample scope is

given for adapting the treatment to the circumstances of the case. For further particulars on the various methods we must refer our readers to the treatises of Liebermeister and Ziemssen, of which the latter especially contains a good deal of practical advice.—*British Medical Journal, Feb.* 3, 1872, *p.* 131.

## 10.—ON THE TREATMENT OF PYREXIA BY A COOLING PAD.

By Dr. WILLIAM ROBERTS, Physician to the Manchester Infirmary.

At a meeting of the Manchester Medical Society, I exhibited, on October 4, a pad designed to apply regulated and continuous cold or heat to the surface of the body. The annexed figure will aid the description. The pad consists of a close coil of very thin indiarubber tubing, three-eighths of an inch bore, cemented to a strong canvas backing. The terminal portions (composed of stouter tubing) reach a couple of feet from the margin of the pad, and each is closed by a stopcock, *B* and *C*. The inner end of the coil pierces the canvas backing at its




*Description of the Tubular Pad.*—The coil is cemented to the canvas backing A A. The stream enters the circumference of the coil at B, and makes its exit at the centre, where the tubing pierces the canvas, and passes along the back of the pad to C, and thence to a vessel on the floor.

centre, and passes behind the pad to its stopcock, *C*. For the purpose of experiment, three sizes were made—one a foot in diameter, another two feet by sixteen inches, and a third three feet by two feet. My principal object in constructing these pads was to obtain a convenient means of applying external cold in cases of fever and inflammation. The observations made in Germany on the treatment of typhoid fever by cold baths, and the more recent trials of the same treatment by Meding and Wilson Fox in the hyperpyrexia of rheumatic fever, seem to hold out hopes that we may be approaching a more effective treatment of pyrexia than has hitherto been attained. The cold bath, ice-water affusions, and iced cloths, are all of them extremely troublesome of application, and wear so heroic an appearance, that any general introduction of them into private practice seems well-nigh impossible. But if some such means of applying cold as I am now proposing could be made effective, it would open an easy way to the study of this method of treatment, and also make it available in private practice. The procedure I have followed has been to place the pad either under the back of the patient or round the trunk next to the skin. A stream of water of any desired temperature is then conveyed through the pad, as it lies under or around the patient's body. This is accomplished by placing a reservoir of water, holding about six gallons, a foot or two above the level of the bed, and connecting this with the stopcock *B*. The most convenient form of reservoir is a tin can pierced near the bottom for a stopcock, to which a length of indiarubber tubing is attached for the purpose of connecting it with *B*. In the absence of a can of this sort, an ordinary toilet-can or large jug may be used, and the water made to flow from it into the pad by a suitable length of tubing arranged as a syphon. When the connection is made, and the stopcocks opened, a stream of water flows through the coils of the pad, and issues at *C*, whence it is conveyed by an exit-tube to a vessel on the floor of the room. In this way a moving sheet of water, of any desired temperature, is brought into near contact with the surface of the body—only separated from the skin by a layer of indiarubber of one-thirty-second of an inch thick.

Before placing the pad under the patient, it is absolutely necessary to fill the coil completely with water. To accomplish this, the pad is placed on the floor of the apartment, near a table some four feet high. The can or jug which acts as the reservoir is then filled with warm water, and placed on the table. Connection is now made by means of the attached lengths of indiarubber tubing, and the current is set a-going, the exit tube being placed in a shallow vessel—such as a wash-hand bowl—on the floor. The empty coil soon begins to fill,

and the air within it to be expelled. The stream should be allowed to run freely for some ten minutes, in order to drive out all the air from the coil. The two stopcocks are then shut, and the pad is ready to be placed under the patient. To prevent a too sudden shock, the pad should be first filled with warm water, and then gradually cooled by adding pieces of ice to the reservoir, or by filling it up with cold water as the warm water flows away.

My trials have already satisfied me that it is easily possible to reduce the temperature of the body by the means here proposed; but whether the method will prove of practical use in the treatment of fever and inflammation must depend on further experience. The subject is of such great and urgent importance, and demands such multiplied and varied experiments to test its utility, that I have thought it desirable to make public my results at once, in the hope that some other inquirers with more leisure at command will take the matter up and institute a more elaborate research.

[A number of tables are then given, showing the effect of the use of the pad on the temperature of the patient. This was always lowered by 2 to 3 or 4 degrees; sufficient for most purposes, but evidently not so effectually or rapidly as by immersion of the whole body in water. The temperature of the effluent water was found to be raised 20° when the stream ran freely, and 30° when it ran more slowly.]

It will be observed that in all the cases the depression of the temperature was only temporary. This has always been found to be the case after the use of the cold bath. It is also seen that as a rule the temperature rises after the withdrawal of the pad to a higher point than before the experiment began. But this appears to be only a temporary jump. The impression on my mind was very decided that the use of the cooling-pad was beneficial on the course of the disease.

It will be interesting to ascertain the effect of a more gradual and more sustained cooling, and to contrast the effect so obtained with the effects of a more rapid and more brief cooling frequently repeated.

In conclusion I must record my obligations to Mr. Buckley, Physician's Assistant in the Infirmary, to whose zeal and accuracy I am indebted for most of the temperature observations.* — *Medical Times and Gazette, Dec.* 16, 1871, *p.* 733.

* The pads were made by Charles Macintosh and Co., Manchester. The price of the medium pad, which promises to be the most useful, is about 35s.

## 11.—APPARATUS FOR APPLYING HEAT OR COLD TO DIFFERENT PARTS OF THE HUMAN BODY.

Designed by Dr. Alexander Robertson, of Glasgow.

These apparatus are constructed for the application of regulated temperatures, from that of ice to the highest that can be borne. They consist of two or more compartments, and have an inlet- and an outlet-pipe from three to four feet in length, through which water is admitted and discharged. The current is regulated by a tap on the discharge-pipe, and may be either constant or intermittent. There is no difficulty in maintaining the temperature of the circulating water to within three or four degrees of a given point. They may be worked either in connexion with a vessel of water which has a tap at the bottom—the end of the inlet-pipe being attached to the top—or on the principle of the syphon, in which case no special vessel is required.

*Head Bag.*—It consists in some of eight, in others of ten, compartments, each a little more than an inch in diameter. These are arranged in parallel lines over the head, those at the sides being much shorter than the central ones. The whole set of compartments are cemented along part of their under surface to thin vulcanised cloth, which fits the head smoothly, and they are covered externally by thin woollen or waterproof cloth, to which thin vulcanised cloth is stitched or cemented along the margin. The cap is secured to the head by straps under the chin. When sufficiently distended it contains only about eight ounces of water; and if a free current be allowed to circulate, this will all be displaced by a fresh supply, admitted by the inlet-pipe, in less than half a minute. Dr. Robertson states that by circulating ice-water, or a solution of ice and salt, cold is more conveniently and efficiently applied to the head than by the ice-bag; and he hopes that by the use of water at definite degrees of heat a soothing influence may be produced on the brain in certain forms and conditions of disease.

*Chest or Abdominal Bag.*—It consists of six compartments, which are enclosed in the same way as in the head bag. When sufficiently distended the weight both of bag and contents is less than that of an ordinary poultice. It may be used alone, or on the outside of a light poultice or fomentation. The heat of the two latter agents may be renewed without in the least disturbing the patient, by simply admitting warm water at the inlet-tube, which displaces what is in the bag when the tap on the outlet one is open. Dr. Robertson says that besides preventing patients from being exposed and annoyed by the renewal of the poultice or fomentation, this bag is useful in certain cases of disease when a uniform high temperature is

maintained for one or more hours continuously, and he expects that it will reduce hyperpyrexia through the circulation of ice-water.

*Uterine Bag.*—It consists of two compartments. On the outer surface is a small pouch for a director to assist insertion; and at the sides and ends are little loops, to which a piece of lint may be tied. It is rolled up lengthways, and can then be pushed up behind the neck of the uterus with the greatest ease. After its introduction, water is allowed to flow into the bag, expanding it, and then passes away by the outlet-pipe. Should a moist or sedative fomentation be desired, the lint is previously soaked with water or other suitable liquid. According to the height of the column of water in the inlet-pipe, the bag may, or may not, be fully distended internally.

Dr. Robertson states that this instrument is useful in inflammatory conditions of the uterus, in certain states of uterine cancer, &c., when warm water is circulated; and that in menorrhagia the cold of ice-water with the pressure of the distended bag, usually stops the bleeding.

*Spinal Bag.*—This bag is about a foot in length, but it may be longer or shorter, and consists of two compartments which communicate freely below. Along their inner margin they are connected closely together, at intervals, by bands. At the top, the one terminates in an inlet-, the other in an outlet-pipe. As in the other apparatus, the current through this bag may be either constant or intermittent. Besides its obvious use in applying either a low or high temperature to the spine in ordinary inflammatory and painful affections, Dr. Robertson thinks that the effect of maintaining an equable high temperature in such diseases as tetanus, locomotor ataxy, &c., is worthy of being tested.

*Throat Bag.* — This instrument exists only in pattern. It is anticipated that it will be found useful in laryngitis, &c.—*Medical Times and Gazette, Jan.* 13, 1872, *p.* 55.

---

## 12.—ON THE TREATMENT OF HYPERPYREXIA BY WITHDRAWAL OF HEAT.

By Dr. T. Clifford Allbutt, A.M., Physician to the Leeds Infirmary, and Lecturer on the Practice of Medicine.

[The water-treatment of fever was first 'invented' in England; it is, however, little known here, although an immense body of experience has been gathered together on the subject by some German physicians.]

I cannot conceive anything more desperately risky than the casual plunging of a fevered patient into a tub on the mere con-

clusion of a single visit, or even of one careful consultation. I feel satisfied, both from analogy and from observation, that those physicians are in great measure right who contend that such rapid withdrawal of heat is followed by a corresponding or a very considerable increase of heat-production. It is not yet familiarly known that the standard of animal temperature does not by any means represent a uniform production of heat, but represents the difference between the amount set free and the amount dissipated. Heat-production probably varies enormously from hour to hour, but heat-dissipation varies exactly as production varies so long as the balance of the organism is steady; in like manner, if we increase the dissipation, we by the same movement increase the production, in virtue of the action and reaction between surface and interior. This, which is true to demonstration in health, holds probably in great measure in disease also; so that when we abstract heat largely, even in disease, we probably increase production in something like a corresponding ratio, if not actually to the same degree. In plunging a patient with a temperature of 106°, say, into water at 60°, we set up an enormous outflow, the body being quickly reduced, say, to 100°; now, the beautiful adjustment of the body being lost, we certainly do not see a continual adequation of heat-production, but instead of numerous and infinitely small compensating oscillations, we seem to get fewer and larger oscillations, so that the organism, when removed from the bath, tends to "react"—that is, to swing back to, and even beyond, its former place. Ziemssen seems to have seen this difficulty more clearly than any of the writers on the subject, and to have drawn from it the practical conclusion that if we are to modify reaction we must modify our withdrawal of heat, and rather coax than compel the organism to enter into an equilibrium. Hence he lays down a rule with which I cordially agree—namely, that the withdrawal of heat must be the slowest possible for the due attainment of our end. We know that a heat of 104° is slowly killing the intimate tissues of the body and arresting molecular movement, and that a heat of 106° is doing this rapidly. It is clear, then, that we must bring the body below this point; but this may be gained by using baths of a temperature far higher than 60°. Theoretically, it would seem sufficient to use a bath of 100°, and to cool it artificially during use to about 96°; practically, however, the temperature falls so slowly as to be scarcely perceptible when the patient is placed in a bath of 100°. I still think, however, that a bath of this temperature, which when fully prepared will stand but little above the normal blood-heat, is the best for a commencement, and that its temperature should then be kept down, and, indeed, slowly lowered during the immersion of the

patient. This lowering must depend upon the state of the patient; but a long immersion and a slow lowering is in all respects, I am sure, more desirable than a short immersion and a rapid fall. If the patient can bear an immersion of an hour, or even of two hours, the lowering may be made very gradually, and need not be carried below 80°. On removal from such a bath, the further diminution of temperature which generally succeeds the immersion will not be followed by a violent reaction—that is, its effects, more slowly induced, will be more permanent. In addition to this treatment directed to increase the dissipation of heat, I always endeavour to prevent its rapid reproduction by administering quinine in repeated doses of from five to twenty grains, according to circumstances, as advocated by Professor Binz.

The next point I would urge is the continued presence of a medical man and the incessant use of the thermometer. No case can really be counted as evidence either for or against the bath treatment in which the thermometer was not watched as incessantly and as anxiously, during the whole treatment, as the steam-gauge of a rotten boiler. To speak nearer to the point; the doctor cannot take an observation, order a bath, and wait to take another until his next visit. The whole treatment must be managed with the utmost precision, and all tendencies to shiver or syncope should be watched by a skilled observer, or irreparable harm may be done in five minutes. No farther reasons need be urged on this important point; but I am satisfied, if more reasons are not wanted, that practical warnings, on the other hand are much wanted, and will be needed more and more if the bath treatment becomes popular. If modern medicine is to be precise, and if its criticism is to be equipped with keener weapons, the awkward truth will begin to stare us in the face that much more will be required of us than the casual routine of the past.

[The following case occurred to the writer last September, and was fortunate in its results.]

Dr.—, a middle-aged man in large practice beyond Keighley, was under the care of Dr. Cockcroft, of Keighley, when I was called to him on account of the symptoms of "cerebral rheumatism," which had supervened upon an ordinary attack of rheumatic fever without heart complication. From the 11th to the 14th of September his state had gradually become very serious; sleeplessness and alarming head symptoms had set in. On the 13th there had been incoherence and delirium; no true sleep for some days; and there was an increasing tendency to coma. Under these circumstances, on the evening of the 13th Dr. Cockcroft had shaved the head and applied large quantities

of ice very freely to the head, forehead, and nape. This application had been followed by a slight but decided improvement. Hitherto no temperatures had been taken. I saw Dr. — on the afternoon of the 14th in consultation with Dr. Cockcroft, Mr. F. Greenwood, and Dr. —'s assistant. The patient was then unconscious and delirious in a low and muttering way, his countenance turgid and oppressed, his pulse 130, and his temperature 105·8°. There was no inflammation of the joints or heart. This condition was reported to me as one of slight improvement, and I had little doubt that the temperature had been lowered by the free use of the ice. While obliged to give the gravest prognosis, I gave my full adhesion to the plan commenced already, and urged, on the grounds before explained, that the withdrawal of heat should be systematically carried out under the guidance of the thermometer. As plenty of ice was at hand, and the use of a bath not so convenient, it was determined to fill wide-mouthed bottles with ice and lay them along with the patient in bed. I think six bottles were so applied at first, and their number modified subsequently according to the temperature. The ice was also continued in a large bladder covering the head and neck, and dripping a good deal about the pillow. After settling all these details and others of less immediate interest, I took my leave. Mr. Greenwood and the patient's assistant remained in the house and superintended all the proceedings, skilled supervision being absolutely necessary in this case. Mr. Greenwood kept and forwarded me the report of its progress, and I here set down extracts from his diary. At 6 p.m. (about three hours after the commencement of the full ice treatment) the temperature was reduced to 102°; at 7 it was 101·4°; at 11 it was 100·2°; and at this time the patient fell into a quiet sleep of four hours' duration, only broken by the occasional administration of food. The bottles were continued more or less according to the fluctuations of the thermometer during the day and night; and I may pass on to Sept. 15th, 6 a.m., when the temperature was 99·4°. At this time the patient was intelligent enough to complain of rheumatic pains. During the remainder of the 15th the temperature fluctuated between 100·2° and 101·2°, the ice being regulated accordingly. He had "a very fair sort of night," and I may sum up the observations of the next five days by saying that the temperature was kept down to points between 100·2° and 100·8°. Sleep was not repeated in the same satisfactory way; there was much exhaustion and therewith restlessness. In addition to the usual stimulants, morphia draughts and chloral were continuously tried to relieve this restlessness, but produced only short dirturbed dozes, with starting, moaning and muttering, and stertorous breathing, It was not thought desirable

therefore to push the draughts. I should have added that during all this time, in addition to the ice bottles, the patient was regularly shifted from one bed to another, and sponged from time to time with cool water. No medicine was taken. On the 19th the symptoms of exhaustion began to pass off, and the hard fight was continued with renewed hope. On the 21st the temperature was easily kept to 99°—100°, and farther improvement was visible. During the next few days less and less ice was used until the 25th, when a steady temperature of 98°—99° was easily maintained without ice, and no more regular observations were made. From the 21st, indeed, the improvement was steady and satisfactory, and the patient entered on the 25th into a convalescence which has since progressed favourably.

In this case, although the applications were very cold, on the other hand their application was partial; nevertheless, I am still disposed to hold that a complete immersion in warm water is better than a partial application of very cold water or ice, except in the coma of hot scarlatina, when douches of very cold water to the head and neck are of great service.—*Lancet, Dec.* 23, 1871, *p.* 881.

---

## 13.—TREATMENT OF HYPERPYREXIA BY BATHS.

Under the care of Dr. Sutton, at the London Hospital.

The governors of the London Hospital, on the suggestion of their resident medical officer, Mr. Mackenzie, have made arrangements, on a scale of great liberality and completeness, for carrying out the treatment of hyperpyrexia by baths. In a small ward, containing two beds, there has been placed a bath, with its head only against the wall, so that a patient can be lifted in and out of it with the greatest facility. This bath is fitted with a large supply-pipe for both hot and cold water, and a rapid waste. The cold water pipe comes from a tank, into which, if need be, lumps of ice can be placed, so as to reduce the temperature with the greatest rapidity. By these arrangements the temperature of the bath has actually upon one occasion been reduced fifty degrees in seven minutes. Not the least of the advantages of these arrangements is the fact that the treatment is robbed of all its terrors, and that the patient is not frightened by an array of buckets and lumps of ice, &c.

We append two cases taken from among Dr. Sutton's patients, which illustrate the mode of treatment. These cannot be spoken of as hyperpyrexial, neither of them having reached that temperature (106° F.) at which, in the opinion

of Dr. Wilson Fox, the cold bathing should be commenced. It is interesting to remark that, in the case of rheumatic fever, a fall of temperature followed immediately upon the bathing; while, in the case of typhoid fever, large doses of quinine apparently accomplished (?) what cold bathing failed to effect. It will be remembered that in Dr. Wilson Fox's remarkable cases, which were both of them rheumatic fever, large doses of quinine utterly failed to reduce the temperature in any appreciable degree. In the first of these cases recorded below slight diarrhœa and albuminous urine seemed upon one occasion to follow as a consequence of the bath.

*Case of Rheumatic Fever with moderately high temperature successfully treated by baths.*—The patient, a girl sixteen years old, was admitted to the London Hospital on September 16th, on the twelfth day of her first attack of rheumatic fever. On admission her condition is thus described by Dr. Sutton: "Patient lies on her back, her mouth open, her eyes staring in a vacant manner; seems to pay no or very little attention to what passes around her. If spoken to she says, in a very excited, irritable manner, 'I want my mother.' Her nostrils expand very much during inspiration. Her breathing is laboured, and her cheeks are very flushed." There was a well-marked mitral systolic murmur. At 3·15 p.m. (her temperature being 104°, pulse 140, respiration 26) she was placed in a bath at the temperature of 96°. She was kept in the bath twenty-five minutes, the temperature of the bath being reduced to 83° When the patient was removed her temperature was 102·9°, pulse 126, respiration 22. When first put into the bath she seemed very comfortable, but when the temperature of the bath was reduced she complained of feeling cold, and cried out a good deal. When removed to bed she said she felt very comfortable; she had lost her wild expression, answered questions rationally, and, closing her eyes, passed off into a sleep. At 9 p.m. the same evening her temperature again rose to 104·4°, pulse 126, respiration 30. She was bathed again as before, and in twenty minutes her temperature was reduced to 102·2°. At midnight on September 17th (temperature 104·1°, delirious and fretful) she was placed in the bath (temperature 98°, reduced to 82°), and when removed her temperature was 102°. When removed from the bath she was observed to shiver. After the bath during the night she passed three liquid dark-yellow stools. On the morning of the 19th her temperature was 101·6°; her general condition had improved; her urine was loaded with lithates, and contained a fourth part of albumen. From that date her condition gradually improved, and on October 17th she was sent into the country.

*Case of Typhoid Fever with high temperature; the bath unsuccess-*

*ful; large doses of quinine quickly followed by a fall of temperature.*—The patient, a woman aged twenty-two, appeared to have reached the eighth day of a mild but well-marked attack of enteric fever. She progressed favourably in all respects until the end of the third week, when the evening temperature, instead of falling, showed a persistent tendency to rise. On the evening of the twentieth day the temperature in axilla was 104·4°, pulse 110, respiration 28. Between the twentieth and twenty-eighth days she had in all nine baths (the baths were similar in temperature and duration to those used in the last case, but the reduction was usually made to 70° instead of 80°). These baths had hardly any effect in reducing the temperature, which, at the most, within a few hours of the bath was as high as it was before. The first bath seemed more effectual than the last. Three-quarters of an hour after her last bath the temperature was 104·8°. On the twenty-ninth day it was 105·9°, and on the evening of the thirtieth 105·5°. The administration of quinine was now commenced in doses varying from ten to twenty grains. A fall of temperature immediately followed the exhibition of the quinine, but the fall was very gradual and slow, and it was not till the forty-fifth day that the normal temperature was reached. With the exception of a slight return of pyrexia on the fifty-third day, which quickly yielded to quinine, the patient made a good recovery, and is now recruiting her health at the sea-side.—*Lancet, Jan.* 13, 1872, *p.* 46.

---

## 14.—THE SYNTHESIS OF ACUTE RHEUMATISM.

By Dr. BALTHAZAR W. FOSTER, Professor of Medicine in Queen's College, and Physician to the General Hospital, Birmingham.

[The facts recorded in the following paper, when added to the arguments which have been adduced by Prout, Richardson, and other writers, will strengthen considerably the evidence which points to lactic acid as the poison of acute rheumatism.]

In the *British Medical Journal* of February 25th, 1871, I read with much interest an account of Dr. Cantani's observations on the lactic acid treatment of diabetes. At that time, I was engaged in completing an inquiry into the effects of different drugs on the sugar-excretion in diabetes. I determined to add one more drug to my list, and to complete my research by observing the effects of lactic acid.

A man (Wright) who had just come into the General Hospital under my care, suffering from diabetes, offered me the opportunity. His age was 31, and he had been ill some four months before his admission. By trade he was an iron-caster, and up to this attack of illness he had been a healthy man, and had

never suffered from rheumatism. He was married, and had several strong, healthy children. On a mixed diet, he passed during the first week of his stay in hospital an average of 180 ounces of urine daily, containing 49 grains of sugar in the ounce. On a strictly animal diet, continued two weeks, the sugar fell to an average of 36 grains an ounce, and the urine passed to an average of 116 ounces daily. The skin was dry and branny. The sugar excretion remained pretty stationary on strict diet, but lung-symptoms began to manifest themselves, and steadily increased.

On March 8th, I ordered the patient fifteen-minim doses of lactic acid dissolved in an ounce of water four times a day. The dose was doubled the next morning, and in the afternoon he complained of acute pains in his joints, and flying pains about his limbs. In the evening, as these pains had increased, the medicine was discontinued by order of the resident medical assistant.

On March 10th, no lactic acid mixture was taken, and the pains gradually ceased.

On March 11th, I saw the case; and, regarding the occurrence of the joint-pains as a mere coincidence, repeated the lactic acid in fifteen-minim doses three times a day. On the evening of the 12th, he again felt pains in his joints; and on the morning of the 13th, "the small joints of the fingers of both hands, the wrists, and, in a less degree, the elbows," were noted by the resident medical assistant, Mr. E. A. Elkington, to have become "red, swollen, and painful." On my visit, I was much struck by the appearance of these joints, which were typical specimens of acute rheumatic arthritis. In the evening, both wrists, the small joints of the fingers, and the elbows were all red, hot, swollen, tender, and painful. The heart-sounds were clear. The temperature in the morning was 100; in the evening, 101 F. He had moderate perspiration. Pulse 90, soft and full. The joints were wrapped in cotton-wool, and the lactic acid was discontinued.

On March 14th, in the morning. there was a decided improvement in all the joints; the swelling had much diminished, but heat and pain were still present. Temperature 100; pulse 84. In the evening, all the small joints of the fingers were much better. The wrists were still affected, and he complained of a good deal of pain in the knees, which had hitherto escaped. The heart-sounds were clear. Pulse 90. Temperature 100·8.

On March 15th, the joints were better. The temperature in the morning was 98·6; in the evening, 99·4.

On March 16th, he said that his arms were quite well; his legs nearly so. He had slept much better.

On March 17th, all pains in the joints were gone. Temperature 98.2. Pulse 72.

During the next twelve days, no lactic acid was administered. The case was put clearly to the man, and, as he had felt benefit from the acid mixture and had passed less urine during its use, he elected to run the risk of acute rheumatism. Accordingly, on March 29th, I prescribed seventy-five minims of lactic acid dissolved in twenty ounces of water. This was to be taken as a drink in the course of the twenty-four hours. During the next five days, no rheumatic symptoms appeared. The pulse rose twelve beats on and after the third day; the temperature, which had been previously elevated by the lung-complications showed no marked change, but on the fourth and fifth days remained steadily at 99°, instead of varying, as it had done for some time previously. On the morning of the sixth day (April 4th), he complained of having had a bad night from joint-pains, which had disturbed him very much, and which came on suddenly after midnight. On examination, the metacarpo-phalangeal and first phalangeal articulations of the first and second fingers of each hand were found to be red, swollen, hot, and painful; the slightest movement aggravated the pain, and he could not on this account pick up anything with his fingers. The pulse was 102. The temperature, which on the previous evening had been 98·2, had risen to 99·4. The heart-sounds were clear. The acid mixture was stopped, and in the evening the pain in the knuckles was less, and the redness had diminished; they were, however, still stiff. No other joints were affected. Temperature 99·2.

April 5th. His hands were much better, and, of his own accord, he resumed his lactic acid drink, and took about thirty minims of acid in the course of the forenoon. In the evening, the pains had returned in the knuckles, which were swollen, red, and tender. He discontinued the acid, had a fair night, and on the morning of the 6th, found his hands free from pain. He again resumed the lactic acid, and took up to 4 p.m. the remainder of the bottle, containing about forty-five minims of acid. In the evening, at 9 p.m., the pain and swelling had returned in his knuckles, and his left wrist was also affected. He now gave up the acid for two days, and the joint-symptoms gradually disappeared.

The acid drink was resumed on the 9th, and continued to the 13th, but he only took about thirty-five minims of acid a day. He experienced no inconvenience, except flying pains about his joints, till the night of April 13th, when he was disturbed by severe pain in the right wrist, which was found in the morning to be red, swollen, painful, and hot, and was a typical specimen of rheumatic joint. Pulse 98, full and soft. There was copious perspiration, of acid reaction. The heart sounds were clear. The elbows and knees became painful and stiff the next

day. The joints were all wrapped in cotton-wool as before; and in the course of four days nothing remained except a little stiffness in the right wrist. After a week's interval, the acid was again taken, with like results.

The man now had gained so much experience as to the first indications of a coming attack in his joints, that he was allowed discretionary power as to the time and manner of taking the mixture. By trying it first in small doses, so as not to take more than twenty minims of acid a day, and stopping it for a day or so whenever the joints threatened, he managed to continue the acid for some weeks. Gradually he increased the dose, as advised, and early in June was able to take from forty to fifty minims daily. During this month, he had two sharp attacks of rheumatism in the hands and wrists. By the end of June he was taking seventy-five minims of acid daily; and on July 6th, this was increased to 100 minims. On the 7th, he began to experience considerable pain and stiffness in his joints, and kept his bed (he had been up daily previously) on account of the pain caused by walking. On the 8th, these symptoms were worse, and in the evening his wrists and elbows were very stiff and painful, but the knees were less so. The temperature had risen to 100·6. The acid was stopped. On the next morning, he was better. Temperature 99. The joints were less painful and stiff; there was no redness and no swelling. On the 10th, he again took the acid, his joints feeling much better, and the temperature being only 98·4. In the course of the day, he took 100 minims of the acid; and by the evening the pains had returned in his wrists, elbows, and knees. Temperature 100·6; pulse 100, full and soft; skin moist and perspiring. On the morning of the 11th, his right wrist was red and swollen; the left less so. The knuckles of his right hand were also red, swollen, and painful. His left knee was red, swollen, and very painful and tender. He complained also of pain in the left side, but the heart-sounds were found to be clear; pulse 88; skin still moist. The mixture which had been stopped on the previous night, was discontinued till July 17th, by which date all the rheumatic symptoms had subsided. After this the man only remained in hospital seventeen days, During this period, he, of his own desire, resumed the acid drink, and on one occasion took as much as 125 grains of acid in the course of twenty-four hours. During the last fortnight of his stay in hospital, he had no severe pains in his joints, and whenever flying pains warned him, he discontinued the medicine for a day.

While the above case was under my care in the hospital, it so happened that another diabetic patient of mine, in visiting the wards, met Wright and compared notes with him. From him he heard such a favourable report of the acid treatment, that

he requested me to order him the same medicine if I thought it suitable. I did so. A drink consisting of seventy-five minims of latic acid in a pint of water was prescribed. Of this he took daily as much as contained thirty to fifty minims of acid; and on the fourth day he came to me complaining of a sharp pain in his right knee, which rendered the joint stiff, and made walking very painful. He also mentioned that he had less severe pains in his other joints, and expressed his opinion that he had caught a cold, which had produced rheumatism, a disease from which he had never before suffered. There was no swelling or redness of the knee or other joints. His skin which had hitherto being harsh and dry, was soft and moist. The acid mixture was discontinued, and in two days the pains had entirely ceased. During the next month, he made several attempts to take the acid mixture, but it was always followed in a day or two by pains in the joints. Early in May, he managed to take the mixture for a week, and then was laid up with such severe joint-pains, that I was called to visit him, and found him in bed with pains in his elbows, shoulders, ankles, and knees, and, as he said, all over him. None of the joints were swollen except the right knee, which was faintly red, decidedly swollen, and very tender and painful. The other joints were simply stiff and painful on movement. The skin was freely perspiring. Pulse 96, full and soft, The acid mixture was stopped, the joints were wrapped in cotton-wool, and alkalies administered. In the course of a week, all the symptoms had disappeared, and the patient was able to walk about, and resume his ordinary habits. This patient had never passed more than twenty-four grains of sugar an ounce while under observation. The excretion was generally not over fifteen grains an ounce.

*Remarks.*—The above record contains an account of the joint-symptoms which were observed in two cases to follow the administration of lactic acid. In the first case, at least six well marked arthritic attacks occurred; in the second case, under conditions less favourable for observation as to duration of treatment and place, one well marked attack occurred. The phenomena corresponded in all respects to those which are characteristic of acute articular rheumatism. They came on when the acid was taken, and ceased when it was discontinued. When moderate quantities of the acid were tolerated, an increase in the dose was succeeded by the painful inflammation of the joints. Coinciding with the development of the articular affection was the appearance of perspiration, at first only slight, but afterwards, in the more severe attacks, copious and acid.

These facts have dispelled the last lingering doubt in my mind as to the truth of the lactic acid theory of rheumatism. At first I doubted the connexion between the administration of

the acid and the production of the rheumatic phenomena. In my scepticism, I regarded it as an accidental combination. The recurrence of the joint-symptoms, however, on March 13th, following distinctly on the repetition of the lactic acid mixture, shook my disbelief. The coincidence of joint-attacks with the use of the drug might occur once, and I thought even a second time; but, when I found it occur over and over again, there was no room left for the hypothesis of coincidence. To refer Wright's attacks to a series of accidental combinations requires, in my opinion, a much livelier faith than to accept the lactic acid theory of acute rheumatism. If to some, Wright's case presents not evidence enough in the beautifully typical character of the artificially produced disease, and in the precision with which it could be manufactured at the will of the experimenter, then the second case comes in to refute any explanation founded on the assumption of an idiosyncrasy on the part of one patient.

In health, no doubt, much larger quantities of lactic acid than any given in my cases would be excreted without producing any perceptible disturbance in the bodily functions. The acid would escape by the skin, the kidneys, or, after oxidation, as carbonic acid and water. It cannot be justly argued that the quantities of acid taken by my patents were too small not to have escaped in this way. The conditions under which the drug was given must be borne in mind. In diabetes we have a state of suboxidation very unfavourable to the conversion by oxidation of new compounds; and in Wright's case this was aggravated by the serious pulmonary complications. Associated with these, there was a dry and branny state of the skin highly unfavourable to the elimination of the lactic acid by one of the common channels. Lastly, the well known persistent acidity of the urine in diabetes points to a pre-existing hyperacidity of the fluids. These considerations are, I think, important, as defining the conditions under which the experiments were made—conditions most favourable to the development of the specific effects of the lactic acid. It was the combination of all these which rendered Wright so susceptible to the action of the drug. By the absence of one of them (the lung-complication), and the minor degree of glycosuria, we may probably explain the slighter susceptibility in the second case. The larger doses of acid which Wright was able to take occasionally, towards the close of his stay in the hospital, find an explanation partly in his more careful management of the remedy, partly in an acquired toleration of it, and partly in the great improvement which occurred under treatment in the state of the respiratory organs and in the sugar-excretion.

I refrain for the present from discussing the bearings of my

observations on the therapeutics of rheumatism. The effects of the lactic acid on the excretion of sugar will be considered, with other modes of treatment, in a future paper. In this communication, my object has been to lay before the profession facts which have an important bearing on the origin of a common and serious malady. If, by pointing out the nature of the poison of acute rheumatism, the help in the smallest degree to improve therapeutics, they will not have been observed in vain. —*British Medical Journal, Dec.* 23, 1872, *p.* 721.

---

## 15.—SOME NOTES ON THE TREATMENT OF SMALL-POX.

By Dr. William Stokes, Regius Professor of Physic in the University of Dublin.

[In reference to the probability of pitting in small-pox we must take largely into account the character of the disease. A case which is sthenic in character is more likely to result in pitting than one which is asthenic, with the same amount of eruption and confluence.]

Looking at the frequency of the occurrence of pitting on the face as compared with that of other parts of the surface, may it not in part be accounted for by the fact that while the rest of the body is kept covered by the bed-clothes, and in a state of comparative humidity, the face remains in a dry and heated condition from the influence of the external air, and the increased vascular action. This we have seen sometimes so intense that no oily application could be used without immediately drying on the surface. Hard and hot scabs were formed, and the suppurative process made its way downwards to a greater or less degree.

It was in the year 1849 that I saw the most severely inflammatory or sthenic case that I have had to deal with before or since. There was great tumefaction, accompanied with extraordinary heat of the face, and, in the hope of saving the eyes, poultices were applied over them. The patient recovered, but with deep and permanent pitting; but at the time when the crusts were formed with their usual dark colour, the countenance presented an extraordinary appearance, the integument of each orbital region being free and almost white, while the rest of the face was everywhere covered with deep black crusts. There was no pitting on the eyelids, or in their immediate neighbourhood. From the date of this case, I have adopted as a routine practice the application of light poultices over the entire face, or of a mask of lint steeped in glycerine and water and covered with a corresponding mask of oil-silk, and have found, but with one exception, that pitting was effectually pre-

vented. In that case the patient was delirious, and could not be prevented from tearing the poultices off his face. I believe that if from an early period we protect the surface from the air, and keep it in a permanently moist condition, marking will seldom occur.

Experience entitles us to hold that, other things being equal, the tendency to pitting, or, in other words, the virulence of the pustulation, is directly as cutaneous vascularity and heat.

This method fulfils three important indications of treatment—

1st, The exclusion of air.

2nd, The keeping of the parts in a permanently moist state so as to prevent the hardening of the scabs.

3rd, The lessening of the local irritation.

I need not remark that the value of the treatment is best seen in the inflammatory or sthenic type of the disease, although in the asthenic or typhoid form the occurrence of marking may be observed, particularly in the confluent cases.

In reference to the importance of lessening the local irritation, the following case, to which I have alluded in my paper, is interesting:—A strong and healthy young woman was admitted with symptoms of an early stage of the ordinary epidemic fever, but of a very active type. The skin was hot and dry, the pulse strong and full, and the tongue loaded. She complained of intense headache, much more severe than that commonly observed in the commencement of ordinary fever, so that I thought it necessary to apply leeches freely to the temples. The headache, heat of the head, and flushing of the face were completely relieved in consequence. Within the next two days small-pox vesicles appeared on the neck and bosom. The case proved a most severe example of confluent variola over the trunk and limbs, while on the face not more than two or three small aborting pustules made their appearance.

Who can doubt that in this instance the depletion of the face influenced the local progress of the disease?

This remarkable case, together with our knowledge of the good effect of poulticing in lessening the irritation, and the consequent pustulation of the part, has led me, even after the vesicles have appeared, to apply small numbers of leeches once or twice to the inflamed skin, and with excellent results. The heat, vascularity, and swelling were much diminished and the pustulation clearly modified. We have here, then, a further evidence *that the activity of pustulation is directly as the inflammatory state of the surface.*

It may be observed, parenthetically, that the influence of local depletion on the development in the part of the secondary lesions of essential disease, may explain the theory, and, in

some degree, excuse the treatment of fever, by the so-called physiological school. The benefit of leeches to the epigastrium and ileo-cœcal regions, so strongly appealed to in defence of the theory, may have been that it lessened the preceding irritation which favoured the follicular engorgement and consequent ulceration of the intestine.

In a paper in the *Quarterly Journal* for May, 1860, a case is given which had been commented on by Dr. Graves. A man was for some time under treatment in the surgical ward for a chronic affection of the knee-joint. I cannot say whether it was or was not rheumatic in character, but it was treated by strapping with mercurial plaster. After some time he showed symptoms of fever, soon followed by the appearance of variolous vesicles. He was transferred to the medical wards, and went though an attack of severe and confluent small-pox. At the period of the *stadium decrustationis* of Hebra the plaster fell off, when the whole region which had been covered by the straps was found smooth and of a glistening whiteness, contrasting strangely with the black and rugged surface of the limb above and below the joint.

In explaining this I incline much more to the effect of the pressure on the cutaneous capillaries than to any specific action of the mercury.

I have come to this conclusion, that in all cases, whether of the asthenic or sthenic form, the local treatment of poulticing is superior to any other. Two great conditions are by it fulfilled—one the lessening of the cutaneous irritation, the other the prevention of the drying of the scabs. It will be obviously more applicable in proportion to the inflammatory state of the surface.

In once case thus treated the result was satisfactory so far as the prevention of pitting over a large portion of the face was concerned, but the tardiness in the falling off of the scabs on the cheeks was remarkable; the character of this case was asthenic or typhoid. The eruption came out slowly, and was not well formed until after stimulants had been used. These, too, had to be employed liberally and for a long period of time. Many of the scabs remained strongly adherent for two months after the first invasion of the disease. As they fell off, however, the skin was found to be uninjured. When portions of the loosening scab were detached, long filamentous processes, very similar to *asbestos*, were discovered running from the under surface of the scab into the skin. At the date of this patient's discharge from hospital a few of the scabs were even still adherent. Could these filamentous processes have resulted from a pathological transformation of the purulent matter?

We have seen that the treatment by poulticing or other moist

applications may be said to owe its success, 1st, to the diminution of the cutaneous irritation; 2nd, to the keeping of the pustules moist; and, 3d, to the exclusion of air. This leads us to the treatment by the warm bath practiced by Hebra, who seems to have been directed to it by observing its efficacy in the management of burns. It is clear that in the case of the continued warm bath we have the conditions just mentioned completely fulfilled, and that, too, as regards the entire person of the patient.

Not many years since one of our students, a very large and robust man, was attacked with small-pox, which soon showed itself in its worst characters. The fever at first was very high, and the head swelling and vascularity of the face intense. The *eruption was universal*, while the pustules on the face became confluent at an early period. Delirium set in, and the patient tore off the dressings from his face so often that we desisted from their further application. After the tenth day the condition of the patient was most appalling. The delirium continued, the circulation became every day weaker and more rapid, notwithstanding the free use of stimulants; the crusts were not only black, but on the legs, where here and there was less confluence, the blackness of the worst purpura appeared—a condition held by Hebra to be always fatal. The body was one universal ulcerous sore, and the agonies of the patient from the adhesion of the surface to the bed-clothes were not to be described. In addition to the usual fœtor of small-pox in the stage of decrustation which was present in the highest degree, there was an odour of a still more intensely pungent and offensive character, which seemed to pass through the bystander like a sword. I never before or since experienced anything similar. Stimulants alone, freely and constantly employed, seemed to preserve the patient alive; the pulse was rapid, weak, and intermitting, and for several days we despaired of his life.

At this juncture I happened to describe the case to my colleague, Mr. Smyly, who suggested the trial of the warm bath, with the view of relieving the terrible suffering. A bath in which he could recline was speedily procured, and pillows being adjusted in it, we lifted the sufferer in and placed him in the recumbent position. The effect was instantaneous and marvellous. The delirium ceased as if by magic; it was the delirium of pain, and the patient exclaimed, "Thank God! thank God! I am in Heaven! I am in Heaven! why didn't you do this before?" The fœtor immediately and completely disappeared, so that on entering the ward no one could suppose that there was a case of small-pox in it. He was kept at least seven hours in the bath, during which time brandy was freely

administered, and omitted only when he showed symptoms of its disagreeing with the brain. He was then removed to bed. The surface was clean, and in many places the sores looked healthy and white. The bath was repeated next day, after which he fell, for the first time, into a tranquil slumber. From this time his recovery was progressive, delayed only by the formation of abscesses and the great soreness of the feet.—*Dublin Journal of Medical Science, Jan.* 7, 1872, *p.* 10.

---

## 16.—THE ANTISEPTIC TREATMENT OF SMALL-POX.

By Dr. Arthur Ernest Sansom, Physician to the Royal Hospital for Diseases of the Chest, and to the North-Eastern Hospital for Children.

[Though, of course, individual instances can have little value as evidence, the following case, to a certain extent, illustrates the fact that the course of zymotic diseases can be modified in intensity and in duration by the internal administration of antiseptic agents.]

I was called on October 13th, 1871, to visit Miss E. T., aged 18. She had high fever, subdelirium, and vertigo. The pulse was 128. Temperature, 105 deg. Fahr. The tongue was red, dry, with brown streak down the centre; the pupils were widely dilated. A few spots closely resembling those characteristic of typhoid existed on the abdomen, but there was no abdominal tenderness, distension, nor diarrhœa. The diagnosis was complicated, for the young lady had lately been staying in a locality wherein typhoid was prevalent. There was no lumbar pain. I gave a guarded diagnosis, but considered it most probable that the case would prove to be one of typhoid. I ordered cool sponging; milk and beef-tea; half an ounce of brandy every four hours; a scruple of sulphite of sodium in solution every four hours. On Oct. 14th, the pulse was 124; the temperature was reduced to 102 deg. Fahr. Several papulæ were present upon the face. The case now assumed the aspect of variola. On Oct. 15th, papulæ were abundant over the face, arms, and legs. On Oct. 16th, all signs of general discomfort had subsided; the patient only complained of irritation of the papules, which began to become pustular. On the 17th, I touched the centre of each pustule on the face (over sixty in number) with a fine camel's-hair pencil, dipped in strong liquid carbolic acid, taking care not to allow any to reach the sound skin, and ordered a solution of one part of carbolic acid in three of olive oil to be applied over the individual pustules night and morning. On Oct. 18th, the patient slept well, all irritation from the pustules having subsided. The carbolic liniment was

continued; the surface of the skin was sponged now and then with oatmeal water. The temperature from this date never rose above the normal; there was no discomfort whatever; the pustules all dried up, and, on the eighth day, a large number had completely fallen off, leaving no cicatrices; the others were quite dry and scaly. On Oct. 23rd, the sulphite of sodium was discontinued, and sulphocarbolate of iron given in five-grain doses three times a day. The surface of the body was washed with coal-tar soap to aid disinfection.

The double principle of the antiseptic method of treatment is the arrest of the disease-process in the individual, and the prevention of spread to the community. The same class of agents which common experience declare to be disinfectants, can be administered to the living body with at least a fair hope of their accomplsshing that destruction of disease-germs which they accomplish externally to it. Carbolic acid has been administered by many, especially by French, physicians. But I believe that, in many cases, carbolic acid and its compounds can be advantageously replaced by other antiseptics—especially in those wherein head-symptoms are prominent. I think it very probable that we shall find certain antiseptics are best suited to the treatment of certain diseases; in scarlatina, diphtheria, and all zymotic ailments in which the throat is involved, I have found the sulphocarbolates specially valuable. In his successful cases of variola, Dr. Hjaltelin employed the ordinary sulphurous acid in fluid-drachm doses every third hour. I have rather inclined to the use of the sulphites recommended by Polli—they are powerful and direct antiseptics, easily administered and readily absorbed. I believe that in this country the error has been made of administering them in insufficient doses, or else of employing the hyposulphites—purgative salts, and far less efficient as antiseptics.

The external treatment of the pustules is most important. No agent seems to me so valuable as carbolic acid; its application in the pure form to the summit of each pustule is perfectly painless. It is not necessary to touch each individually at one visit, but at subsequent times to touch those which have been omitted previously. I have found nothing so effectually disguise the odour of carbolic acid, without impairing its antiseptic efficacy, as oil of wild thyme (oleum origani). Thymic acid is itself a well-known antiseptic. The effect of the application of the carbolic acid is at once apparent; the pustule first becomes white, and then dries up. The carbolic oil afterwards applied, penetrates amongst the purulent crusts, and is far more efficient than any watery application. The general surface of the body may likewise be sponged with any soluble antiseptic. I believe the coal-tar soap to be very valuable for

washing the surface of the body. By the antiseptic method of treatment, external as well as internal, the patient is really disinfected from the onset of his malady, and the benefits are manifest alike upon himself and upon those subject to the contagion.—*British Med. Journal, Nov.* 25, 1871, *p*. 611.

---

## 17—ANTISEPTIC TREATMENT OF SMALL-POX.

By Dr. ARTHUR WYNNE FOOT, Physician to the Meath Hospital.

Dr. Foot has recently communicated to the Medical Society of the College of Physicians, Dublin, during their discussion of the subject of small-pox, a paper which we deem worthy of being made as widely known as possible. A full report of the proceedings of this Society appears in the current number of the *Dublin Monthly Journal*, to which our readers may refer, should they desire greater detail. The subject is the "Antiseptic Treatment of Small-pox," and is the more important inasmuch as it introduces a principle into the chaotic varieties of treatment generally adopted. The ordinary plan of dealing with small-pox, as with most other diseases which run a course, is to let it alone, to treat symptoms as they arise, and to ward off evil effects should they make their appearance. In the ordinary sense of the word this is not treatment at all, and we are by no means sure that it is sound in principle. At all events, Dr. Foot has attempted, with a certain degree of success, to apply a system which has been attended with good results in Surgery to a loathsome disorder, and one of extreme severity. We are happy to make his ideas and his system more widely known than they now are, and we hope that others will test both, so that we may know their true value.

The total number of patients treated was fifty-nine; but these were so carefully studied that their value is much greater than a multitude less closely observed. Of the fifty-nine; twenty-four were confluent and six semi-confluent, and out of these eleven died—a mortality of more than one-third. But it must be remembered that confluent small-pox in a severe epidemic is an extremely fatal malady, its fatality amounting to quite 50 per cent; moreover, the cases treated were in badly fed patients, brought late to Hospital, some unvaccinated—in short, a most unpromising series. Dr. Foot says—

"The way in which I endeavoured to carry out the antiseptic treatment was by giving carbolic acid internally in the shape of the sulpho-carbolate of sodium, and when more suitable the sulpho-carbolate of iron, giving the sulphurous acid of the Pharmacopœia, diluted with water, as the usual drink, using

gargles of sulphurous acid, spraying the larynx, with it, washing the nares and upper surface of the soft palate with solutions of sulphurous or of carbolic acid, keeping carbolic oil to the face, washing the body with solutions of sulphurous acid or vinegar and water, throwing pure sulphurous acid about the bed and bedclothes of the patient, and burning sulphur in the room, so that the sick might breathe for some portion at least of the day an atmosphere charged with some sulphurous acid gas in it.

"After much consideration of the subject I have adopted the opinion that the secondary fever of confluent and semi-confluent cases is due to the presence in the body of products of decomposition, which commence to be formed as soon as the lymphy contents of the hitherto vesicles become purulent, rather than that it results from the dermatitis which springs into existence at that period, and which I consider to be the necessary consequence of the irritation of the now numerous sub-epidermic abscesses; and believing that in carbolic acid, used both externally and internally, there is an agent capable, when it can get fair play, of checking the decomposition of the pus, or of paralysing the effects of the products of its decomposition, I considered the first thing was to ascertain the best mode of its administration.

"I have given the sulpho-carbolate of sodium, in thirty-four cases of small-pox, in doses of from seven grains occasionally, to sixty grains every third hour; it is very soluble, and can be taken in plain water, or if its earthy-saline taste must be disguised, it can be given with some infusion of orange-peel or of cascarilla. During its administration carbolic acid is eliminated by the lungs, its odour being very perceptible in the breath, and the sulphuric acid and soda pass off by the kidneys. I have not observed it to cause any sickness of the stomach or unpleasant feeling in the head, even in very large doses; children have no objection to it. I have also verified the observations of Dr. Sansom, that subsequent to its administration the fetor of the evacuations from the bowels is greatly lessened, the urine is unusually slow to decompose, and the flesh resists putrefaction. I had opportunities of remarking the latter fact in making post-mortem examinations. I never found anyone to complain of the usually nauseous variolous odour of the skin.

"At the same time, in all confluent and semi-confluent cases I use sulphurous acid in every form and way in which it can be applied.

"One of the simplest and most effective ways of exhibiting sulphurous acid is in the gaseous form; flowers of sulphur dropped on a heated shovel and carried about the room with its pale blue flame, forms by its combustion sulphurous acid

gas, which, diluted with the nitrogen of the atmosphere, can pass into the lungs of the patients; and I consider that there is reason to believe that this practice three or four times a day is beneficial to the attendants and other inhabitants of the house as a prophylactic. Irritation of the bronchial membrane soon gives notice when there is as much sulphurous acid gas in the atmosphere of the room as is consistent with health; it specially and soonest affects anyone with bronchitis, and I should say it ought to be used very carefully if the variolous patient were labouring under that affection. One of the nurses at the Hospital who suffered from chronic bronchitis used to be greatly affected by the daily fumigations if in the rooms when it was being done, and used to feel its effects long before and long after any of the patients or other attendants. Some persons in health also have by idiosyncracy a condition of the lining membrane of the air-passages which makes them peculiarly sensitive to this gas, even in a very much diluted form, and in such a reflex cough is very quickly excited. It is not to be supposed that the principal object in burning sulphur is to disinfect the room—this is an after-consideration, and would require an amount of sulphurous acid gas dangerous if not fatal to life; but it is to develope as much of this antiseptic agent as may be safely inspired, with the view of checking the multiplication of the small-pox poison in the person of the patient, very minute quantities of sulphurous acid being capable of arresting fermentation.

"The sulphurous acid of the Pharmacopœia undiluted I frequently apply in an atomised vapour to the nares and pharynx, through vulcanite tubes, curved or straight as may be required, in the manner recommended by Dr. Dewar, of Fifeshire. I have found that patients like it, and eagerly ask to have the operation repeated. It removes disagreeable tastes from the mouth, keeps the nose free from obstruction by accumulated crusts, and much of it must reach the lungs. A few whiffs open the nose when it is stuffed, or when, as hospital patients say, the head is stopped; the spray has not the suffocating odour of the bottled acid.

"I give them the acid internally several times in the day, or for a drink both day and night when there is much thirst, in drachm or two drachm doses at a time, diluted with water. One drachm of the acid in two wineglassfuls of iced water is a very pleasant drink. Less water may be used—a drachm may be taken in a wineglassful of water; but if the acid has been freshly prepared, or very well kept, the drinking such a solution may catch the breath. If the person drinking the acid in the strength of one drachm to two ounces of water will avoid inspiring through the nose when the glass is brought to the lips,

and swallow the liquid in gulps, the vapour cannot irritate the air-passages. I always have the acid added to the water which the confluent and semi-confluent cases drink, and they like its acidity very much. I frequently take it myself as an agreeable and wholesame beverage, and advise the students to do so, as a prophylactic. I prefer the sulphurous acid to the sulphites or bisulphites, because they are so unstable in solution—the form in which they must be given—rapidly absorbing oxygen, and passing to the state of sulphates. Neither have they much to recommend them in preference to sulphurous acid on the score of cheapness, purity, or flavour."—*Medical Times and Gazette, April* 6, 1872, *p*. 400.

## 18.—THE HYPOSULPHITE OF SODA IN VARIOLA.

By Dr. William A. Corwin, Assistant Surgeon U. S. Navy, U. S. S. Benicia, Asiatic Squadron, Harbour of Yokohama, Japan.

[The value of the internal antiseptic treatment of zymotic diseases has, we believe, been clearly demonstrated. It was the knowledge of the action of sulphites in arresting fermentation out of the body which first led to Prof. Polli's experiments on their action on fermentation within the body.]

The U. S. S. Benicia arrived in the harbour of Yokohama, Japan, November 22, 1870. At Yokohama, and at other prominent Japanese seaports, small-pox may be said to be *endemic*, occasionally prevailing among the foreign residents, and then assuming an epidemic form. It has so prevailed there during the past winter to an unusually severe extent, causing considerable mortality not only among the permanent foreign residents, but also among the English and French troops stationed there, and the merchant and navy shipping in port. We were destined to have our share, the first case occurring in a seaman on the 18th December, 1870.

The patient was at once separated from the rest of the crew, until arrangements could be made for transferring him to the General Hospital ashore, where the case proved confluent, and resulted fatally in a few days. Measures were at once taken to secure a suitable building for our own hospital purposes, and thus have our sick under the immediate care of our own medical officers.

A second case followed on Christmas day, and the disease gradually progressed until sixteen of the ship's company, including two wardroom officers, were "on the list." The cases were of more than average severity, four proving fatal. Of the latter all were confluent, three of the patients being men of

intemperate habits, and the fourth, a negro, who died during convalescence from the disease, from a sudden attack of œdema of the lungs.

A large roomy dwelling-house, selected with excellent judgment by Surgeon H. C. Nelson, U. S. N., and situated on the bluffs to the rear of the city, had been secured as a temporary hospital, and thither the cases were transferred as fast as they declared themselves, beginning with the second.

On the occurrence of the third case it occurred to me to try the effects of the *bisulphite* of soda; but this drug could not be obtained. So, with the concurrence of Surgeon Nelson the *hyposulphite* was employed in drachm doses, and we had every reason to be gratified with the result. This treatment, used in the premonitory fever *only*, was commenced with the *fourth* case, and its effects carefully watched. They were those of an *alterative*, mild *hypnotic*, and *laxative*; its administration being in most cases followed by a subsidence of the fever, a tardy or incomplete development of the eruption, and relaxation of the bowels with watery stools. Upon the full development of the eruption the remedy was generally discontinued, and a supporting *regime* adopted, egg and brandy mixture, with easily digested food. The good effects of the salt were generally manifest after the first dose; the patient losing the heat and dryness of the skin, expressing himself as much more comfortable, and passing a good night. In two or three of the cases the eruption was delayed from twelve to twenty-four hours after the usual time for its appearance, and in one patient the eruption consisted of irregular erythematous patches with successive crops of minute vesicles in the flexures of the limbs. The average duration of the first three cases (fatal) was six days. Of the cases that recovered one was *malignant*, the rest of all degrees of severity. Their average duration was *twenty days*.

What I particularly wish to have noted is the fact, that in direct proportion to the early and free use of the remedy, really harmless for evil while so potent for good, was the disease ameliorated and its average duration shortened, and this in an epidemic of more than usual severity.

It may be objected that the cases enumerated are too few to establish the value of the remedy. This is more than is claimed for the results given. My only object is to add a trifle to the evidence constantly accumulating in the columns of the various medical journals as to the value of sulphur and its lower combinations with oxygen in the treatment of the zymoses, and to elicit, if possible, from the profession at large the results of wider observation and experience. (*Vide* note on topical use of Sodæ Sulphis in Erysipelas, by Dr. Addinell Hewson, Transac-

tions Coll. Phys., Phila. 1867; also note on Sodæ Hyposulphis as a Prophylactic in Scarlatina, Dr. N. L. North, Aitken's Practice, Am. Ed., page 318).—*New York Medical Record, Aug.* 15, 1871, *p.* 267.

---

### 19.—DISINFECTANT TREATMENT OF SMALL-POX, FOLLOWED BY DISINFECTING BATHS.

By J. N. Stephens, Esq., Plymouth.

Having lately had charge of two small-pox hospitals in Plymouth, I have adopted the disinfectant treatment of Dr. Sansom, of London, viz., the administration internally of bisulphate of soda every four hours, and the application of olive-oil and carbolic acid to the pustules externally. By these means, he contends, that "the patients are themselves disinfected, and rendered innocuous to the community at large." I must say this plan has been attended by more favourable results than any other with which I had previously been acquainted, and can most strongly recommend it to my professional brethren. I have, however, to make certain that no infection should be conveyed to the public after leaving the hospital, caused each patient to be put for a quarter of an hour on three successive days into a bath containing a pint of chloralum. This has been done immediately convalescence has been established, and the patients have been discharged from the institution forthwith. By this proceeding much expense has been saved, and the beds made available for other sufferers in quick rotation. I have also employed chloralum in solution as an external remedy before the application of the olive-oil and carbolic acid. This has been the means of cooling the inflamed surface and of allaying the itching.—*British Medical Journal, Feb.* 3, 1872, *p.* 127.

---

### 20.—BALSAM OF COPAIBA IN SMALL-POX AND SCARLATINA.

By Dr. A. Rowand, Quebec.

From our knowledge of the effects of the balsam on the skin and mucous membranes, I was induced to try it, in four- or five-drop doses, mixed in ℥ ij. syrup, and ℥ ij. mucilage of gum arabic, three or four times a day, in the confluent small-pox of a person who had never been vaccinated. It caused no nausea, but, on the contrary, created a keen appetite, which continued till recovery. No pitting took place, and no local application was used but glycerine and water. I tried the same mixture in scarlet fever, with most satisfactory results. Under its use, the tongue and sorethroat got rapidly clean and well, followed by a

keen appetite, and by none of the usual sequelæ. The secretion of urine was copious, and began to increase in quantity after two or three doses. At first it was of the colour of ale and a little ropy, but by the third day quite clear and normal. My theory of the action of the remedy is, that it alters or destroys the character of the virus, and eliminates it out of the system by the skin and kidneys more particularly ; for the recoveries have been unusually rapid. In both cases I prescribed milk, beef-tea, wine, and spirits, according to need.—*Medical Times and Gazette*, *Feb*. 17, 1872, *p*. 206.

---

## 21.—THE CORRELATION OF VARICELLA AND VARIOLA.

By Frank Thorp Porter, Esq., Dublin.

I communicate the particulars of two cases which illustrate the correlation of varicella and variola. I consider that the two diseases are more intimately related than has been hitherto admitted by pathologists. I shall also offer some remarks on the treatment of variola generally, including the subject of re-vaccination ; and I shall conclude with some suggestions on a subject of public interest. I allude to the question of the speedy establishment of a suitable building for a convalescent home for small-pox patients, who suffer much from the want of such an establishment.

The first case is that of Ellen Lawlor, æt. 14, who was admitted into the South Dublin Union small-pox sheds on January 13th, 1872. She was sent in by my friend Dr. Mapother, who diagnosed the case as one of varicella. On admission she was weak, her countenance was dull, the pulse was 90, the bowels were confined, and the tongue was white and moist. The primary vaccine marks were visible. A few isolated vesicles, which wanted the areola and pitted appearance of small-pox, presented themselves on face, neck, and arms. The only treatment resorted to was a mild purgation and generous diet with porter. She left hospital well on the 22nd of January, 1872.

The second case is that of the late porter at St. Vincent's Hospital, Michael Byrne, æt. 37, who was admitted on the 17th of January, 1872. He stated that he felt in good health up to the 13th of January, and that after being in communication with the patient Lawlor, he was seized with headache and shivering fits, followed by loss of appetite, sleeplessness, pains in the bones, and heat of skin. On admission his pulse was

100, tongue of beef-steak colour, conjunctivæ were suffused, bowels constipated, urine scanty and high coloured. An erythematous rash appeared over the body, followed by vesicles, which soon became confluent, and purpuric, giving exit to sero-bloody pus. The only treatment consisted of a purgative, wine, and beef-tea, with a sinapism to the loins. His intellect was clear up to January 21st, when he became so delirious as to require restraint. He died on the following day. The fact that he became suddenly ill after being in contact with the previous patient, clearly suggests that varicella may communicate variola. This is an important question for investigation. I regard the two diseases as modifications of the same blood-poison; an opinion which is strengthened by the fact that they often appear in the same family, and sometimes in the same person.

With reference to the treatment of variola, every effort should be based on the principle of isolation. A number of warm, well-ventilated huts would carry out this object better than sheds. Mild cases could be better treated at the patients' homes. Rigid enforcement of the Sanitary and Vaccination Acts, combined with a more extended system of out-door relief, would limit the spread of infection. I do not coincide in Professor Haughton's opinion that the use of spirits is prophylactic in epidemics. Teetotallers recover soonest. Increased facilities for taking baths should be placed at the disposal of the poorer classes, with a view to prevention. I have but little faith in drugs, although I think the muriated tincture of iron is beneficial in some cases. Flour or starch is a better application for the face than other preparations which cause stench. The use of brewers's balm lessens the tendency to abscess. I was led to try it from seeing its effects in anthrax; it seems to prevent putrescency. The bed sheets ought to be made of some carbolised material, such as the tissue used by Professor Lister in wounds.

With reference to re-vaccination, I have no faith in it. Not one of the thirty-six attendants at the South Dublin Union sheds have taken small-pox. Only seven of the number were re-vaccinated, and as the remaining twenty-nine enjoy the same immunity, wherein is the necessity of the operation? I have known gouty inflammation, abscess of the breast, and angio-leucitis to result from the operation. I cannot, in the face of such facts, approve of it, and moreover the sense of the Profession is against it. It is only to be employed when there is no evidence of the success of infantile vaccination, and even then it seems to do more harm than good; at least so far as I have seen.—*Med. Press and Cir., March* 27, 1872, *p.* 269.

## 22.—TREATMENT OF CHOLERA BY SUBCUTANEOUS INJECTION OF MORPHINE.

By Dr. JOHN PATTERSON, Surgeon-Superintendent of the British Seamen's Hospital, Constantinople.

In an article in this journal on the cholera in Cairo in 1865, I expressed regret at not having tried the subcutaneous injection of medicines, having special reference to quinine. A recent severe epidemic in parts of this city and at Hasskieu, a village on the Golden Horn, the residence of a large English colony, has given me an opportunity of trying the effect of the subcutaneous injection of morphine on a sufficiently large scale to judge of its value. The first cases were treated by the usual remedies; everything rational was tried, and with the usual want of success. Completely disheartened at the inutility of treatment, I went prepared with the instruments and morphine, and, after consultation with my colleague, Dr. Wherry, determined to give it a trial. A most unpromising case was selected. The man had been previously suffering from inflammation of the liver, was in deep collapse, pulseless, with rice-water purging, severe vomiting, and cramps. I injected a quarter of a grain of acetate of morphine. The result was beyond our expectations. In a quarter of an hour the cramps and vomiting ceased, the patient fell asleep, the skin gradually became warm and moist, the pulse returned. In two hours he awoke, and said he felt much better. The injection was repeated; he again slept for three hours. The reaction was perfect. He lived three weeks, and sank from typhoid exhaustion, as much produced by his old liver complaint as from the reactionary fever. The same good results followed in almost every case in which it was tried. In ordinary cases one or two injections of from one-quarter to one-half of a grain sufficed. In a few cases three injections were given, and only twice have I had occasion to give four. It was given even to very young children in doses proportioned to their age and condition. I speak more of well-marked cases. In the milder form, where the purging, vomiting, and cramps were severe, and collapse just commencing, we never wasted time on other treatment. An injection of a quarter of a grain was given, perfect quiet enjoined, and bottles of hot water placed in the bed. The patient fell asleep, and, as a rule, awoke nearly well. Many cases were thus nipped in the bud. I do not, of course, maintain that this treatment is a specific against cholera ; I only claim for it that its action is more decided than any other treatment I have seen or practised, and that in the race against death we gain time for further treatment when it is necessary. It is long since I lost hope of the ordinary treatment influencing much the course of

the disease, and, after treating more than a thousand cases at various times of epidemic, I am glad to report that this has really been of great service. My colleague, Dr. Wherry, speaks equally favourably of the results obtained in his practice. I regret that want of time prevents me at present from giving the cases in detail; but the subjoined table gives at a glance the main results :—

| | Number of Cases. | Recovered. | Dead. |
|---|---|---|---|
| Treated in the usual manner . . | 10 | 1 | 9 |
| Treated by morphine injections . | 42 | 22 | 20 |
| Total . | 52 | 23 | 29 |

But of the cases treated by injection, 8 were perfectly helpless from the first, being *in articulo mortis*, 1 had severe liver complaint, and 1 was far advanced in consumption; so that, in reality, we had 32 cases where the treatment had a fair chance, reducing the mortality to 10 in 32; and of these ten, 1 was 60 years of age, 1 within a few days of her confinement, and 3 hard drinkers.

In reference to the subject of cholera, I may here state that I am engaged in a series of experiments on animals, the result of which I hope soon to communicate. So far I have subjected dogs to the action of cholera-matter from the human subject. Injections under the skin, into the rectum, and feeding them with food sprinkled freely with cholera dejections have given negative results.—*Med. Times and Gazette, Jan.* 27, 1872, *p.* 95.

---

## 23.—ON THE EMPLOYMENT OF NITRITE OF AMYL IN THE COLLAPSE OF CHOLERA.

By Dr. T. Lauder Brunton, Joint Lecturer on Materia Medica, and Casualty Physician, at St. Bartholomew's Hospital.

For the purpose of relaxing the contraction of the pulmonary capillaries, nitrite of amyl would seem to be the very remedy. Dr. George Johnson observes that nitrite of amyl may not act on the pulmonary capillaries as it does on the systemic ones. I have, however, tried it on animals, and find that it does. When given to rabbits, it causes the systemic capillaries to dilate enormously, the blood flows rapidly into the veins, and the pressure on the arterial system sinks in a corresponding degree. If it did not produce dilatation of the pulmonary as well as of the systemic capillaries, the blood which pours rapidly into the veins could not pass with equal rapidity out of them through the pulmonary vessels, and it would consequently accumulate

in, and distend the right side of the heart. In order to see whether this was the case or not, I thoroughly narcotised a rabbit with chloral, put a canula into its trachea, and kept up artificial respiration. I. then opened the thorax, and, after carefully noting the appearance of the heart, passed the vapour of nitrite of amyl, mixed with air, into the lungs. When this was done, the cardiac pulsations become a little quicker, but not the slightest distension of the right side of the heart or of the jugular vein could be observed. I repeated the experiment several times with the same result. There is no reason to suppose that the chloral with which the animal was narcotised altered in any way the action of the nitrite of amyl on the pulmonary vessels, for I have found that other vapours, as well as certain poisons, injected into the jugular vein cause accumulation of blood in the right side of the heart after the animal has been completely narcotised. I have also observed that this accumulation of blood, which I believe to be due to contraction of the pulmonary vessels caused by the poison or vapour, has disappeared in the case of one poison at least, after the injection of another drug, which had been previously found to be an antidote to it in other respects. With the prosecution of these experiments I am at present engaged; and I take this opportunity of thanking Dr. Burdon Sanderson for his kindness in allowing me the use of his laboratory and apparatus.

Dr. Johnson also says that, even if nitrite of amyl were shown to possess the power of relaxing the pulmonary capillaries in health, it might not do so in disease, or at any rate not to the same extent. Some observations which I have made on its action in angina pectoris lend probability to this idea. In the paper already referred to, which I published in 1867, I mentioned my belief that angina was due to spasmodic contraction of the capillaries; and in a paper in the Clinical Society's *Transactions* for 1870, I gave copies of one or two of the sphygmographic tracings which led me to form this belief. In it I noticed that occasionally after the anginal pain had disappeared from every other part of the cardiac region, it remained persistently at a spot two inches inside the right nipple. This local persistence of the pain was accompanied by a peculiar condition of the pulse, which seemed to me to indicate that the pulmonary capillaries had not become relaxed to the same extent as the systemic ones. As spygmographic tracings, especially when taken with an instrument like the one I used in which the pressure exerted by the spring could not be estimated, sometimes admit of different interpretations, I append copies of some of them. I may mention, however, that these tracings are strictly comparable with each other, although they may not be so with tracings taken at a different time;

for while these were being taken, the instrument remained attached to the arm of the patient, and the pressing of the spring which pressed upon the artery remained unaltered. When the nitrite was inhaled, the pulse in most cases quickly regained its normal character; but when the pain remained in the region of the right nipple after the inhalation, the arterial tension fell as usual, indicating dilatation of the systemic capillaries, but the volume of the pulse remained small. The small volume is, I think, due to the pulmonary capillaries not dilating, or only imperfectly, under the action of the nitrite; and if I discontinued the inhalation before the volume of the pulse became normal, as well as its character, I felt nearly certain that the spasmodic pain would speedily return with all its former intensity.

The observation of Drs. Hayden and Cruise that the pulse became perceptible in their patients after the inhalation, seems to show, however, that the nitrite did relax the pulmonary vessels during the collapse, and allowed a fuller stream of blood to pass into the left ventricle, and thence to the body. Why, then, did it increase the difficulty of breathing? One would have thought that, by allowing more blood to flow through the lungs, it would at least have alleviated this symptom, if it did not remove it altogether. The experiments of Dr. Arthur Gamgee at once enable us to answer this question. In a paper in the Royal Society's *Transactions*, he showed that nitrite of amyl combines with hæmoglobin, the oxygen-carrier of the blood, and prevents it from giving off oxygen after it has taken it up. When the blood is exposed to the vapour of the drug as it passes through the lungs, the oxygen becomes locked up in it, and cannot be given off to the tissues for their use, so that however much oxygen the blood in the arteries may contain, it acts very much as if it were venous. In consequence of this, I have found that, when the vapour of the nitrite is passed into the lungs of rabbits, they are seized with suffocative convulsions. The greater part of the blood which passed through the lungs having been acted on by the vapour, it affects the brain just as it would have done if the trachea had been closed, and no air allowed to come near it during its passage through the pulmonary vessels, and therefore convulsions occur as soon as it reaches the brain. Professor H. C. Wood, of Philadelphia, who has lately written a valuable paper on the action of the nitrite, found no convulsions after injecting it subcutaneously. By giving it in this way, the blood was not acted on during its passage through the lungs, and asphyxia was prevented. This, I think, shows that if nitrite of amyl is to be used at all as a remedy in cholera, it must be given internally or by subcutaneous injection, and not by inhalation. This mode of ad-

ministration seems to me to be indicated also in spasmodic asthma, when the arterialisation of the blood is also impaired. In angina pectoris it will act when given internally, but a larger dose is required than when inhaled; and it remains to be seen whether it can be given in cholera in sufficient doses to relax the capillaries without, at the same time, injuriously impairing the respiratory changes in the blood and tissues.—*British Med. Journal, Jan.* 13, 1872, *p.* 44.

---

## 24.—ON THE THERAPEUTICAL IMPORTANCE OF RECENT VIEWS OF THE NATURE AND STRUCTURE OF CANCER.

By Henry Arnott, Esq., Assistant-Surgeon to St. Thomas's Hospital.

A calm survey of the two opposite views of the nature of cancer, and of the facts which are adduced in support of them, seems to leave us in about this position:—that while it is impossible absolutely to deny the existence of a special dyscrasia as the cause of the appearance of malignant tumours, the evidence in its favour will not bear careful scrutiny, although it may be admitted that certain individuals and even families exhibit a remarkable predisposition to the occurrence of these growths, just as a similar predisposition may be shown to the appearance of fatty growths, warts, or other such local defects of nutrition; that owing to appreciable physical causes—as the special arrangement of structural elements, the part of the body attacked, &c.—certain tumours are more prone to malignancy than others: that whether the original growth be absolutely local or depending on general changes in the system, a stage may be at length reached in which there may be, indeed, an ineradicable and necessarily fatal blood contamination: and finally, that as all the real evidence before us goes to prove that it is in any case the *local tumours or ulcers which kill*,—either by invasion of vital organs, or by the breaking-up of health caused by excessive hemorrhages, pain, and exhausting and fœtid discharge from the primary seat of the disease—these should be completely removed when possible.

Of course the zeal and hopefulness with which the surgeon sets about this task will depend in a great measure upon the particular theory which he may hold, but it seems illogical to contend that because a man believes that profound constitutional changes have caused the appearance of a terrible local mischief, therefore he need not trouble to attempt the removal of the dire result of a cause which is beyond his control. Even if he feels confident of the system of his patient being satura-

ted with the poison, he will yet endeavour to obviate the more tangible mainifestations, whenever this can be done without incurring a yet greater risk.

If, on the other hand, the surgeon be persuaded that all the formerly mysterious phenomena of cancer are now explicable by local changes readily appreciable to his senses, and to a great extent under his control, he will arduously seek for some means by which to eradicate the malady in its more harmless stage.

In the endeavours to establish a sound therapeutical application of these modern doctrines of the pathology of cancer, one most comforting assurance is at once presented to us. It is this: We have seen that malignancy is by no means the exclusive property of one form of new growth, but it is shared to some extent by nearly all the tumours with which we are acquainted. We have further seen that the degree of malignancy appears mainly to depend upon such physical conditions as the minute structure of the tumour, its consistence, and the amount of moisture and movement of the affected locality. Clearly, then, we need not isolate cancers from the other new growths in the selection of our remedial measures. There is no apparent reason why a remedy, which will remove an innocent tumour, will not be equally efficacious, as regards its action upon local overgrowth, when applied to a malignant one; any circumstances (as extensive local infiltration, or early gland contamination) likely to interfere with the successful issue, may be readily taken into consideration, and the treatment and prognosis varied accordingly.

Hence there is nothing specific distinguishing cancerous from other tumours which should prevent us from applying other methods of treatment to such cases as for any reason happen to be ill-suited for operation. To speak of the knife, as some distinguished surgeons have done, as the only trustworthy resource for the cancerous patient—and this, too, in the face of the unsatisfactory results hitherto achieved by this means—and to rank with quacks those who earnestly seek for better remedies, is calculated to advance neither our knowledge of disease, nor our faith in therapeutics.

Lastly, there is no reason in the world why we should refuse to admit that a cancer has been cured or permanently removed. It has become quite common to hear surgeons say of a tumour which has not returned after extirpation some years previously, "I suppose it was not really cancer, as it has not come back, but it had certainly all the ordinary characters of cancer." In like manner it is frequently said of reputed cures of cancer by pressure, or caustic, or by other means, that the tumours could not have been cancerous simply *because* they disappeared under

the treatment. It is surely time that we give up these notions of the specific and inevitably mortal character of malignant tumours, for such doctrines are not only to be deprecated on account of the insufficiency of pathological facts to be urged in their support, but for the far more serious reason that they stand in the way of any advance in rational therapeutics.

Nevertheless, when we come to consider the question of treatment more carefully, complete removal of the tumour with the knife is the first and obvious expedient of the surgeon, and indeed, where it can be safely and effectually accomplished, this seems to be the distinct indication. But it is unfortunately the fact that complete removal is only certainly possible in a very early stage of the disease.

*Partial removal* is clearly inutile—if not absolutely hurtful—from any point of view, save under very exceptional circumstances.

But since patients seldom seek advice until the earliest stage has already passed, what is now indicated?

Still, as it would seem, complete removal if possible; but now a large amount of apparently healthy tissue must be taken away with the tumour and any infected glands must be extirpated at the same time.

This is the operation which is most frequently called for, and the defective performance of which is to be blamed for much of the opprobrium attaching to surgical interference in cancer.

It is, however, very important to bear in mind that this operation, if effectually done, is often necessarily hazardous to life. How hazardous we have no statistics to show, for its danger has been misrepresented by the invariable custom of grouping together in the same statistical statements these larger with the comparatively trifling incisions of less permanent value, or which are sufficient in an earlier stage of the disease.

This point has been always overlooked in quoting the mortality from the operation of amputation of the scirrhous breast, and the consequence has been that a variety of useless operations have been added to a few effective ones with the result of lowering the rate of mortality of the whole.

But this grave error, which has deceived surgeons by permitting them to under-estimate the risks to life of these operations, is not the only evil which has resulted from this custom. The profession being thus guided by the results of operations clearly inadequate for the removal of the complaint (as when enlarged glands are left in the axilla, or portions of the breast or thickened skin are left behind, not to speak here of the strange practice of intentionally removing *most* of a tumour only, and leaving the nipple or hardened muscle to spread the disease again), a great and unjust prejudice has been excited

against the use of the knife at all, the erroneous impression of the small risk to life being overweighed by the almost constant return of the disease after a longer or shorter interval.

It is difficult to over-estimate the importance of these considerations, for they strike at the root of all our present practice for the relief or cure of the most terrible disease with which the surgeon has to grapple. Taking the most favourable view of the nature of cancer—that it is an absolutely local change, but extending far beyond its obvious limits, and very prone to disseminate itself widely unless removed at an early period—it is yet evident that we have to combat a malady which will yield to no half measures. The knife which is to cure it must cut widely and deeply to effect its purpose. There must be no piece-meal removal which may scatter the seeds of the disease in the wound; nor must any the least suspicious bit be left behind. The practice of sponging into the fresh operation wound a solution of chloride of zinc, as first suggested by Mr. De Morgan, seems also to be strongly indicated, unless the surgeon prefers to use carbolic acid, or any other of the numerous substitutes which, under the title of "disinfectants," have been zealously advocated since chloride of zinc was first used in this way at the Middlesex Hospital. It will do no good to shut our eyes to the gravity of such an operation. Let its increased risks be set against its greatly increased efficacy, and if the risks are found to outweigh the benefits, then let other means less dangerous be sought.

It is probable that in a large proportion of the cases coming under the surgeon's care, some less immediately dangerous therapeutical measure might be profitably substituted.

Caustics have seemed to some to meet this end to a great extent, and their employment is certainly attended with infinitely less risk to life than extensive cutting operations. The intolerable and enduring pain attending their use has hitherto contributed mainly to their neglect by English surgeons. From some reason not easy to comprehend the practice of congealing the part before the application of the caustic, although found to be completely effectual in the instances in which it has been tried, has not yet been sufficiently widely tested to permit this combination to be ranked amongst the reliable measures for the relief of cancer; but much remains to be done in this direction, more particularly in those cases where the disease is conveniently located for such an application, and where the patient has an unconquerable dread of the knife.

No doubt any remedy which affects the system generally must, to some extent, affect any local ailment which may be present, and in this sense general remedies may be of some service in cancer. But it does not seem likely that we shall

readily succeed in finding a drug whose action shall be so concentrated upon a given limited portion of the body as to arrest in that part the excessive and ill-directed cell proliferation which is so vigorously going on. It seems to be a peculiarity of all morbid growths (as distinct from mere inflammatory tissue changes), that they flourish quite independently of the general nutrition of the body, and at the expense of the normal tissues from which the necessary pabulum is diverted.

Failing, then, general remedies, we should seek such a measure as will aim at correcting the local growth, by supplying such condition or combination of conditions as are found to retard physiological growth and development.

The present despairing custom of anointing the surface of the swelling or ulcer with soothing unguents, or of fomenting, or of the application of the various bland lotions or liniments in common use must be regarded as mere *placebo* treatment—useful, indeed, in its way for the alleviation of some of the aches and pains of the growth, and for the calming of the minds of the sufferers, but unworthy the serious consideration of pathologists who, holding either of the theories of the nature of the disease, yet hopefully seek some rational means of removing the local mischief.

For we may again remind ourselves that the mystery of the nature of cancer has almost entirely passed away. That we still know nothing of the proximate cause of the appearance of a malignant growth is an admission we may fearlessly make, since we know so little of the proximate cause of most of the morbid changes whose results are now so minutely studied by the morbid anatomist. But we no longer vex ourselves with the fruitless search for a specific cell-form or chemical test which shall distinguish cancers from other tumours. If our microscopic researches have taught us nothing more, at least they have taught us this,— that the most malignant tumours differ from the most innocent and benign only in certain physical conditions of structure or position which vary greatly in different specimens, and which by their variations seem to explain with sufficient clearness the corresponding varieties of malignant properties.

More than twenty years have passed since Dr. J. Hughes Bennett published his work on Cancer, with its able and (considering the period at which it was written) heretical chapter on "rational treatment," and to-day we may still learn many a valuable lesson from its perusal. The operation which that distinguished physician on purely pathological grounds, so warmly advocated, we on the like ground, and strengthened by greatly extended histological researches, still contend for as the most certain means of eradication of the disease where it can be

sufficiently boldly performed. Ablation by means of caustic has been vastly improved since then, and now forms one of the recognised modes of treatment of certain cancers.—*St. Thomas' Hospital Reports*, 1871, *p.* 115.

---

## 25.—ON INHALATION, FOMENTATION, DISINFECTION, &c., WITH MEDICATED STEAM AND VAPOUR.

By Dr. J. Williams, Malvern.

[When medicated vapours are used with the ordinary inhaler, too rapid cooling takes place. Some means of maintaining the heat is required in order to get the full benefit of this mode of treatment.]

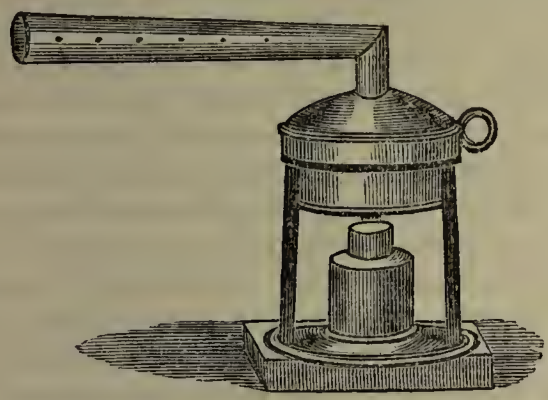
A more complete investigation of the subject has enabled me to construct an inhaler, with a lamp, which fulfils nearly every possible requirement for all parts which we have mentioned, and certainly combines many points of importance unattainable by any other system with which I am acquainted. My invention enables the Practitioner to introduce nearly all the substances and fluids which could be given by the atomiser, and at the same time continues the inhalation of all vapours and medications at a uniform heat to the lungs and breathing surface, fomenting, as it were, the whole mucous lining of the air passages, which has a very important and soothing influence. The influence upon the nervous system is equally certain in spasmodic cough; and in nervous asthma, especially, the action is very striking and satisfactory. I may mention that volatile substances, such as camphor, carbolic acid, hyssop, anthemis, &c., are peculiarly efficacious and agreeable when introduced in the form of vapour, which, by the arrangement I have proposed, does not in any way interfere with the digestive organs. In fact, if any doubt could possibly exist as to the action of medicated inhalations upon the system, the common daily use of chloroform, nitrous oxide, methylene, &c., would answer the question in the affirmative. Besides, the introduction of poisonous gases—hydrocyanic and carbonic acid—are well known to be rapidly fatal in this mode of administration; therefore, by a parity of reasoning, we may assume that the introduction of vaporised medications must, if pursued and investigated, become an extremely valuable means for the introduction of numerous medicines into the human body.

In localised neuralgia, face-ache, ear-ache, &c., a jet of steam directed against the part affected gives almost instantaneous relief, entirely superseding fomentations, mustard and other burning plasters; these latter remedies, in many instances (in my own experience), serve only to increase the original pain. After the

action of the steam-jet, the part affected may be gently rubbed with chloroform, aconite, or camphor liniment, to prevent undue action upon the skin by damp, cold air, &c.

As a disinfecting and fumigating agent I find this lamp superior to all other means, as the chemicals are converted into subtle vapour which seems instantly to destroy unpleasant odours in the sick-room, and in a far more perfect manner than by sprinkling the fluids or powders about the room.

*Directions for Using the Inhaler or Fomenting Lamp.*—Having first fixed the wick in the lamp, pour into it about an inch deep of methylated spirit; then in the pan over the flame place about half a tumbler of warm water, and in the perforated plate a few herbs, such as hyssop, senega, camomile, pennyroyal, &c. These may be used several times without renewing. The apparatus now being ready for use, light the spirit-lamp, and as soon as the water boils a stream of medicated steam will issue from the spout. As the steam would be too hot next the spout, it is better to use a tin or paper tube, expanding to about two inches at the open end. This tube has holes in it to allow



*Lamp-inhaler made by Mr. Sparkes, Malvern.*

the atmosphere to mix with the steam. By this arrangement, and by turning the holes round, the temperature may be very nicely regulated. As the steam ascends rapidly, the open end must be held below the mouth or part to be acted upon, and at a little distance from it. A folded piece of paper from the tube resting on the nose, or a loosely rolled tube in the mouth, in some cases is an extra advantage, and excludes the vapour from the eyes, if desired; but in many cases it is of most service when inhaled rather hot. In using carbolic acid and some other ingredients it is better to add them to the water before applying the lamp-flame to it. The tube used for conveying the steam has holes in it; these should be turned round to the lower side, but, if too hot, brought half round, and if required still cooler, to the upper side.—*Medical Times and Gazette, December* 9, 1871, *p.* 704.

# DISEASES OF THE NERVOUS SYSTEM.

## 26.—ON THE HEALTHY AND MORBID ANATOMY OF THE PERIVASCULAR SYSTEM OF THE BRAIN.

By W. W. WAGSTAFFE, Esq., Joint Lecturer on Anatomy at St. Thomas's Hospital.

[What is known as the perivascular system was discovered by Robin, in 1855, and the only two subsequent investigators in this field have been Professor His, of Basle, and Dr. Bastian, who translated Professor His's paper, and appended his own observations upon it.]

So little notice has been taken generally of the subject, that I am glad the occurrence of three peculiarly interesting cases gives me the opportunity of offering the following remarks upon what must be considered a most important physiological structure.

*Healthy Anatomy.*—The perivascular canals are seen with the greatest ease in the fresh brain. They are sometimes visible even with the naked eye. Remove a small vessel from the substance of the brain by means of a fine pair of forceps, and it will be seen upon examining it under the microscope, that ensheathing the wall of the vessel is a very transparent membrane, broader in some parts than in others, and distinguished from the vessel by its almost stuctureless character, and by the absence of the ordinary contents of vessels. In some places the membrane lies close to the vessel-wall, so that it is with difficulty detected; in other places a large space exists between them; and, as a general rule, it may be stated that the size of the perivascular space increases with the size of the vessel.

That this membrane forms a complete canal around the vessel is certain, from the result of examination microscopically, and also from the result of injection. The space is so well defined, and the membrane limiting it so distinct, and its occurrence is so constant when ordinary care is used in the examination, that it would be useless to question its existence. Moreover, Professor His has been able to inject the system, and has shown that everewhere these canals surround the blood-vessels, and that they are quite sharply defined externally.

The structure of the sheath is peculiar, and the description of it given by His, and Robin, and Bastian, differing as they do from one another, cannot be considered altogether correct. Robin and Bastian describe it as a delicate hyaline membrane in its normal condition, but His states that it is composed of a striped basis-substance very similar to the walls of the splenic veins. Bastian allows that it frequently undergoes a fibroid

change, but he apparently considers this the result of some morbid process, and not its constant and normal condition in any part.

It may be easily shown, however, that the structure of the sheath differs very considerably in different parts, and that a description of a sheath surrounding a small vessel will not at all apply to that enclosing a large one. If a small vessel be examined after removal from the human brain ten or twelve hours after death, the wall of the sheath appears to be structureless, or only faintly granular, when a one-sixteenth object-glass is used, and no epithelial lining is visible. But, if such a vessel be examined immediately after death, the appearances are different. The transparent membrane is lined by a very definite layer of epithelium, composed of cells of very unequal sizes packed together closely, and containing nuclei which occupy nearly the whole of the cells. Both the cell outline and the nucleus are extremely faint, and no space appears to be left between this epithelium and the wall of the vessels; where the cells are large, the perivascular canals are bulging, and where they are small they project but slightly. The transparent membranous wall too gives indication of a transverse and longitudinal striation.

When a rather larger vessel is taken, evidences are then obtained of a somewhat different structure. Examined from the human brain some hours after death, the sheath is seen to be marked by faint lines branching and anastomosing, starting here and there from more definite fibres, and now and then there appears a faint nuclear corpuscle embedded in the wall, and connected with the fibre-markings on every side. The intermediate wall appears still almost structureless, but I have noticed both in the fresh preparations and in those which have been treated with nitrate of silver, that there is a tendency to a transverse striation sometimes very marked, but usually difficult to trace. I have been unable to detect any connection between the fibres of the wall and the exterior of the vessel within, and Prof. His states positively that "no connection exists between the wall of the vessel and the wall of the perivascular canal."

A similar vessel being examined next from a dog immediately after death, it was found that the walls were composed of an outer set of longitudinal wavy fibres, inside which was a circular set of delicate but distinct branching fibres. Inside this again was a quantity of corpuscular material of which I was unable to trace the anatomical relations.

Lastly, when a large vessel is examined, the structure of the perivascular wall is again different. It is composed of bundles of wavy fibres, similar to if not identical with those of white

fibrous tissue, and no trace of the structureless membrane remains within this fibrous longitudinal layer. Indications still exist of the transverse markings, but they are much more obscure than in the smaller vessels, and they are only at all readily traceable in the perfectly fresh brain. The examination of the larger sheaths is difficult, and it is not easy to determine the existence or absence of epithelial lining, or of the connection between the wall and the exterior of the contained vessel, although such connection appears to me highly probable.

Perivascular canals exist without question throughout the whole of the brain. They may be traced, as I have said before, upon any vessel pulled from the substance of the brain. But it is not so easy to follow them in the membranes, and there is reason for it.

I may finish these introductory remarks by alluding to the probable function of these canals, and in doing so it will be necessary to bear in mind the fact that, although so distinct in the central nervous organs, they are extremely difficult to trace in the majority of tissues, and that in many textures their existence is at the most only a matter of conjecture. Where they are most easily seen, as in the brain substance, there is no surrounding connective tissue, whereas in the structure in which they are either absent or their existence is doubtful, loose connective tissue surrounds the vessels. It seems, therefore, probable that the fluid which exudes from the blood for the purposes of nutrition, particularly in tissues undergoing rapid nutritive changes, such as nerve substance, and, possibly, muscular and gland tissues, is held in these canals ready for immediate use, whereas in other structures the fluid which exudes is held in the abundant spongy connective tissue which is so evident in these situations. Add to this that in the case of the central nerve organs we have in the perivascular canals a means of reducing to a minimum the mechanical effects of alternate dilatation and contraction of the vessels, and of protecting the delicate nerve substance from the more gradual alterations in pressure which might otherwise result from the sudden, extensive, or permanent alteration in the blood supply. A further function might be suggested in their influence upon the vessels themselves. In principle these are floating within the perivascular sheaths, and the chances of rupture or other injury from concussion are thereby materially diminished. The physiological function of the contents of these canals is, therefore, probably identical with that of the cerebro-spinal fluid with which it is in direct communication. It may be looked upon, in fact, as both protective and compensatory.

*Morbid Anatomy.—Case* 1.—The first case which I have to mention is that of a child who died at the age of a year and a

half of tubercular meningitis in November, 1869. There was a family history of phthisis. The mother had suffered from phthisical symptoms before and at the time of the child's birth, and had died of this disease when the child was a few months old. Since the child's death an elder sister has also died of phthisis. He had the usual symptoms of cerebral meningitis, and died at the end of rather more than three weeks after the first appearance of the symptoms.

The *post-mortem* appearances coincided with what was suspected before death. Tubercular deposits were scattered freely over the surface of the brain, and a large mass of similar structure was found occupying the fissure of Sylvius on the right side. The tubercle was always related to the vessels definitely; on the upper surface of the hemispheres clusters of miliary deposits were arranged along the sides of the larger vessels: in some cases the naked eye could trace small vessels running into single deposits: in the large mass in the Sylvian fissure the carotid and the middle cerebral arteries were surrounded by the new growth: and it is worthy of notice that both carotid arteries were very markedly thickened before they entered the brain. On slicing the brain it was found to be congested, as were the membranes outside, and there were small points of extravasation in the ventricles, as there also were rather frequently in the membranes. Everywhere the brain substance was rather soft, but the distinction between the white and gray matter was well defined; the softening was, if anything, rather more marked on the right side.

The relation of the deposits to the vessels has just been referred to above; but when they were examined microscopically a much more beautiful and exact relationship could be traced. To explain this I will premise by saying that both the large and the small deposits were composed of nucleated corpuscles—of nuclei, in fact, surrounded by an irregular amount of plasma, the nuclei being of pretty uniform size. In some places they were mixed with a large quantity of granular and oily matter; sometimes with the so-called compound granule cells—these additions being, of course, only the evidences of degenerative changes. Now, when a portion of the membrane was examined, which was but little obscured by the large masses of deposit, but in which it was still easy to see the small miliary tubercles standing distinct, the first point which attracted attention was the marked distension of the perivascular canals. These, which are often difficult to demonstrate in the meninges in health, were now very distinct, and distinct from being distended with abnormal contents. They contained corpuscles exactly similar to those found in the masses of tubercle. Sometimes there were many particles of oily and granular matter, and other

evidences of degeneration, and such appearances were usually more marked in the neighbourhood of the larger masses of deposit. But, besides the diffusion of the elements of tubercle in these perivascular canals, and their great abundance near the masses of the deposit, further evidence was obtained of the relation between them. One of the very small miliary deposits was examined, and showed that it was situated entirely within the perivascular sheath. The sheath was bulged considerably by it; and although the large amount of molecular and oily matter prevented the artery itself being seen, yet it may be fairly inferred from the evident accumulation of tubercle corpuscles between the vessel and the sheath in other parts, that such miliary deposit was here situated in the same position—that is to say, simply in the perivascular space.

This specimen was taken from the pia mater, and affords, it seems to me, conclusive evidence of the existence of the perivascular system here.—*St. Thomas's Hosp. Reports*, 1871, *p.* 149.

---

## 27.—ON NERVOUS OR SICK-HEADACHES.

By Dr. P. W. Latham, Physician to Addenbrooke's Hospital, Cambridge.

[The pathology of nervous or sick-headache is a defective supply of blood to some portion of brain, owing to contraction of one of the cerebral arteries, probably the middle cerebral. There is generally loss of tone of the cerebro-spinal system, from overwork, anxiety, or some similar cause. The headache is frequently preceded by a glimmering of some portion of the field of vision of one eye. If the patient will lie down this glimmering not unfrequently passes off or becomes much less intense, and the headache which would have followed is averted or correspondingly modified.]

Let us consider separately the remedial measures to be adopted (1) during the stage of disturbed sensation, (2) during the stage of headache, and (3) during the intervals between the attacks.

1. *During the Stage of Disturbed Sensation.*—In the forms attended with disturbance of vision, you will find that in the same individual the longer this stage lasts, the greater will be the headache; and therefore we must endeavour to shorten it as much as possible. If the condition, then, depend upon deficient supply of blood to a part, such means must be adopted as shall assist and increase the flow of blood to the part; and this can be done in some measure by posture and stimulants. Directly the glimmering appears, the patient should lie down with the head as low as possible, and if the glimmering be on

the right or left of the field of vision, he should lie on the *opposite* side. Let him take at once a full-sized glass of sherry; if at hand, half a bottle of soda water is a useful addition. Champagne would be preferable, being more diffusible; but its administration would often involve a little delay, and at the commencement of an attack it is a great point to save time. A large tablespoonful of brandy diluted may, if the patient prefer it, be substituted for the sherry. If alcoholic stimulants be objected to, or if it be not advisable to recommend them, then a teaspoonful of sal volatile in water may be prescribed instead. If the patient be chilly or his feet cold, the couch should be drawn before the fire and a hot bottle applied to the feet. By these means the heart is enabled to drive the blood with greater force to the brain, and the duration of the vibratory movement is thereby materially lessened. After it has passed off, the patient should lie still for a time, so that the glimmering may not return. This injunction will only be necessary when the headache is slight; if it be severe, attended with much nausea or vomiting, the patient will be little disposed or able to leave the recumbent position. If, instead of the disturbance of vision preceding the headache, there be a feeling of depression or irritability, fidgets, &c., the administration of such cerebro-spinal stimulants as henbane, valerian, assafœtida, spirit of chloroform, or ether, will often cut short the attack; ten or fifteen drops of the tincture of henbane with the same quantity of spirit of chloroform, will soothe the nervous irritability in the slighter forms, and may be repeated in three or four hours, if necessary. If there be great mental depression, then valerian or assafœtida should be tried. Stillé says: "Nothing is more astonishing in the operation of remedies than the promptness and certainty with which a dose of valerian or assafœtida dispels the gloomy visions of the hypochondriac, calms the hurry and agitation of nervous excitement, allays commencing spasms, and diffuses a soothing calm over the whole being of one who but an hour before was a prey to a thousand morbid sensations and thick-coming fancies of danger, wrong, or loss." I give the preference to valerian, and prescribe from half a drachm to a drachm of the ammoniated tincture. The assafœtida may be given in the form of the spiritus ammoniæ fœtidus of the *Pharmacopœia*, also in half-drachm or drachm doses. As a rule, alcoholic stimulants are not advisable here. A small quantity will cause flushing, heaviness, slight confusion of thought, &c., without relieving the depression; and though the severe headache may be averted, alcoholic stimulants do not answer so well as the remedies previously mentioned.

2. *During the Stage of Headache.*—If the headache be slight and the patient soon able to sit up, there is little to be

done; a cup of coffee or tea, cheerful conversation, a walk, drive, or ride, may often help to remove the pain. If, however, the headache, nausea, &c., be severe, then the administration of further remedies is called for. The patient should keep perfectly still and quiet, with the room darkened; for every sound or sight causes pain, and the slightest movement is sufficient to produce gastric uneasiness. Sometimes free evacuation of the contents of the stomach, especially if it contain undigested food, is followed by relief. Dr. Fothergill says, "an emetic and some warm water soon wash off the offending matter and remove these disorders," which may be very well where there is any offending matter to wash off, but it is not very often that this is the case; the nausea frequently continues long after the contents of the stomach have been discharged; an inverted action of the duodenum is set up; the bile appears in the fluids excreted; the patient believes that all his troubles are due to "its overflow;" "it's all liver," he says, and it is sometimes difficult to persuade him to the contrary. Generally, then, you should try relieve and check the vomiting. Iced soda-water, with or without two or three drops of dilute hydrocyanic acid, or spirit of chloroform; cold tea; the effervescing citrate of potash, with hydrocyanic acid, may often afford marked relief. The headache may be lessened by applying cloths dipped in cold water, or evaporating lotions to the head; if the extremities be cold and the headache severe, a warm stimulating foot-bath can be tried so soon as the nausea will allow the patient to sit up. If the attacks occur in the early part of the day, as soon as the pain has subsided, it is generally better for the patient to sit up, or move about, or take exercise in the open air. A young lady, on consulting me for this disorder, said: "Nothing relieves these headaches except a good galop on my pony. I have sometimes to lie still for three or four hours before the pain is bearable; but directly I am able, I mount my pony and always return home better." During the attack the appetite is diminished, the idea even of food provoking disgust. Still, after the nausea has passed away and the headache has continued a few hours, a plate of soup or some easily digested food will often have a good effect in equalising the cerebral circulation. A remedy which may very often be given with advantage if the headache be severe, is bromide of potassium, in doses of 5, 10, or 15 grains, to which 30 or 40 minims of sal volatile may in some cases be added with advantage; and if the nausea still continue, these may be given in combination with the effervescing citrate of potash. A saline purgative at the commencement of an attack is sometimes an effectual remedy; but, as a rule, the use of purgatives is objectionable.

So far, the measures which I have suggested are only pallia-

tive. We come now to the consideration of such as are preventive, or to the treatment necessary during the intervals between the attacks. First of all, you must try to find out the exciting cause and endeavour to remove it. Hours of study or work must be abridged; excessive bodily fatigue, loss of rest, everything in fact, must be avoided which the sufferers know from individual experience will act as exciting causes. Where the attacks are associated with excessive mental work, they should be regarded as danger-signals, showing necessity for relaxation. In the next place, you must endeavour to improve the tone of the bodily and nervous systems by proper medicinal and hygienic means; and the chief remedies which I employ are steel, strychnine, and cod-liver oil. The success, however, following these remedies depends a great deal upon the way in which they are administered. For a day or two after the attack the stomach and bowels may possibly be disordered, and not in a fit state to tolerate such remedies. This must first be corrected. The simple vegetable bitters, such as gentian, with small doses of henbane and some aromatic, may be of service, and, if necessary, one or two grains of blue pill, with four or five of compound rhubarb pill, may be given at night. We may then try steel. If the attacks have been very frequent, or if there be any scrofulous tendency, I give the iodide of iron in the following form. ℞. Ferri et ammon. citrat. gr. v; potassii iodidi gr. ij; aquæ ℥j; and I add, according to circumstances, 15 to 20 minims of tincture of henbane, or 20 to 30 minims of aromatic spirit of ammonia. If the stomach be at all irritable, I give this in the effervescing form, adding to each dose 20 grains of bicarbonate of potash, and directing it to be taken with a tablespoonful of lemon-juice or a corresponding amount of citric acid: the dose to be taken twice a day, about 11 and 4. I soon leave off the effervescing form, and then add to each dose five minims of liquor strychniæ, omitting the henbane and sal volatile, and continuing the iodide of potassium according as it seems to be indicated or not. In other cases, I give the citrate of iron and ammonia with strychnine at the beginning, and sometimes combine them with infusion of calumba. The iron is indicated by the greater or less anæmia of the patient; but the strychnine is, in my opinion, a very important remedial agent in the disorder. In small doses it acts as a simple tonic, increasing the appetite and improving the digestion; it dilates the vessels, and thus increasing the supply of blood, it augments the activity of the spinal cord (Harley). It promotes the capillary circulation, and therefore its use is advisable for persons troubled with cold hands and feet (Anstie); and if it fulfil these conditions, it is clearly indicated in the disorder which we are considering. Cod-liver oil

also often acts very beneficially. "It has been found by experiment that great exertion and prolonged labour can be endured without fatigue when starchy and fatty foods are alone eaten . . . and there is reason to think that cod-liver oil is more easily absorbed than other similar substances" (Ringer). "It improves the digestive process, increases the proportion of red corpuscles in the blood, and invigorates the whole nutritive function" (Wood); and I believe it particularly sustains the energy of the brain during prolonged mental exertion. A gentleman in the foremost rank at the bar told me that, whenever he was engaged in a jury-trial which was likely to tax his energies to a greater degree than usual, the thing which best sustained him was a good dose of cod-liver oil taken in the morning before going into court; and others engaged in mental work have confirmed this view. I therefore regard cod-liver oil as having, besides its other properties, a nutrient and tonic action on the cerebro-spinal nervous system. As a remedy for these nervous headaches, I only prescribe it once a day, beginning with a small teaspoonful immediately after breakfast, and gradually increasing the quantity to a tablespoonful, but not beyond, unless in exceptional cases.

You must take care to regulate the action of the bowels, but by no means have recourse to strong purgatives. Five grains of the Socotrine aloes pill, given at night, are generally sufficent. If the bowels be habitually constipated, then no remedy seems to answer so well as the aloes and iron pill. Five grains given twice a day, half an hour before meals, will act freely; and in a few days you will have to diminish the dose, for the remedy possesses this advantage, that its effect is augmented instead of being lessened by continual administration, especially when strychnine is given at the same time. The natural waters of Friedrichshall or Marienbad may in many instances be of service, given as laxatives.

Besides the remedies to which I have called your attention, others have been recommended, such as arsenic and quinine, caffein, &c. Where anæmia is not a prominent symptom, they may sometimes be of service.

Lastly, you must lay down stringent rules for your patients with regard to diet and exercise, and you must impresss upon them the importance of these rules being strictly observed.—*British Medical Journal, March* 30, 1872, *p*. 336.

---

## 28.—ALCOHOLIC PARAPLEGIA.

By Dr. Samuel Wilks, Physician to Guy's Hospital.

[Cases of paraplegia resulting from an excessive use of alcohol are stated by Dr. Wilks to be not at all uncommon. All medical

men are doubtless familiar with the occurrence of epilepsy from this cause. But paraplegia must be, except in the large experience of such a man as the writer of this paper, very rarely seen.]

"*Drunkards' or Alcoholic Paraplegia.*—I do not know that this is deserving of a distinct name from its possessing any pathological peculiarities; but as arising in connection with a very well marked exciting cause, it may deserve your especial attention, and I refer to it the more especially because I believe authors have generally overlooked it. I have already told you how long-continued habits of intemperance in alcoholic drinks tend to the production of a fibrous or fatty degeneration of the various tissues of the body, and that, as a consequence, the membranes of the brain and spinal cord become thickened, and the organs within wasted. This, of course, would give rise to what might be called a general paralysis of body and mind. But besides these general results, we often meet with more direct effects on the spinal cord, and to these I particularly refer. I have now seen so many persons, especially ladies, who have entirely given themselves up to the pleasures of brandy-drinking, and been rendered paraplegic, that I have become pretty familiar with the symptoms. From what we hear of our continental neighbours, it would seem that the diabolical compound styled 'absinthe' is productive of exhaustion of nervous power in even a much more marked degree; it would appear that the volatile oils dissolved in the alcohol give additional force to its poisonous effects. Of course, drunkards of all descriptions suffer from nervous and muscular weakness; but, as I before said, it is more especially in the legs that the effect is most striking. A loss of power is first observed, accompanied by pains in the limbs, which might indicate a chronic meningitis of the spinal cord, and in some cases there is anæsthesia. There is, at the same time, some amount of feebleness of other parts of the body as well as the mind, and thus an approach to general paralysis is produced; but sometimes the symptoms are almost confined to the legs, and resemble in character those of locomotor ataxy."

Since this was written I have seen several cases of a similar kind; and it is worthy of remark that they occurred mostly in women. I would repeat that something more definite is intended by the term "alcoholic paralysis" than that general muscular and nervous debility which is as well known to the public as the profession; for no character is more easily recognised on the stage of the theatre than the victim of chronic drunkenness. That which I wish more particularly to draw attention to, is the case where alcohol is seen exerting its influence more directly on the spinal cord, making paralysis the leading

symptom. Although there is a tendency to a particular form of degeneration in alcoholism, it is not very evident why one person should be affected in one way and another in another; or why one person should have cirrhosis of the liver, another Bright's disease, and a third atrophic meningo-cerebro-myelitis. I use this term since the changes which the brain and spinal marrow undergo are probably identical with those which are found in the two other affections just mentioned. As regards the brain, the tissue degenerates and the membranes become thickened, and thus the mental condition of the "brainless sot" is familiar to all. It very often closely resembles that observed in the general paralysis of the insane; which is not remarkable, seeing that the two affections may have in some cases a similar pathology. Now, in alcoholic paraplegia there is every reason to believe that the spinal cord is affected in the same way as these other organs, and the following is the usual condition of the patient:—She lies in bed or on a couch, complaining of severe pains in all the limbs, more especially in the lower ones, which are much wasted, or of a sensation like electric shocks running through them, together with numbness and considerable anæsthesia, and at the same time only slight power of movement, or total inability to stand. With the addition of the akinesia, the symptoms are not unlike those of ataxia, which in its ordinary form appears to be comparatively rare in women. In one case there was hyperæsthesia. In nearly all the cases the liver has been enlarged; there has been sickness, and all the other usual signs of chronic alcoholism. I alluded in my lecture to the existence of pains in the limbs from which drunkards often suffer before any marked signs of paralysis have shown themselves.

As regards treatment, this is hopeful, and should always be attempted; for I think it must be confessed that it is impossible to tell what kind and amount of change has occurred in the centres to produce paralytic symptoms. If the patient be young and the tissues not much degenerated, I should recommend the ordinary treatment, and especially such medicines as the iodide or bromide of potassium, before commencing galvanism and a tonic course. What, however, I would especially insist upon is the removal of the primary cause of the complaint. This seems a common-sense recommendation; but, I am sorry to say, is one not always enforced. Sometimes the reason is that the doctor fears for his position as health-proprietor of the family should his hints be offensive, or he has not moral courage to unfold an unpleasant truth. Sometimes, however, he will most conscientiously refrain from recommending a discontinuance of the alcohol for fear of the results, such as the sudden dissolution of the patient, or an attack of delirium

tremens. He need not have the slightest fear on these grounds, for no harm, but only good, will ensue from its withdrawal. I am aware that opinions are at variance in this matter, but nevertheless I enforce my own views strongly, with a large amount of experience to support me. I have never yet seen a person die or suffer from delirium tremens or any other disorder in consequence of the complete withdrawal of alcohol when the system was being poisoned by it. On the other hand, I have seen such remarkable recoveries after its total discontinuance as would certainly surprise those who had never ventured upon the plan. I have seen persons, more especially ladies, lying on their beds surrounded by friends waiting for their last moments, where scarcely a mouthful of food had been taken for months, where the prostration, increased by urgent sickness, was extreme, and where they were constantly plied with brandy to keep them alive a little longer, and yet in such cases, where the diagnosis was clear, from the blood of the patient being overcharged with poisonous elements, and the room stinking with the fetid vapours of the body, I have recommended a withdrawal of every drop of the so-called "stimulus," and with effects more marvellous than anyone who has not adopted the plan can conceive. Unfortunately these cases of alcoholism are so bound up with moral considerations, and in women so often associated with bodily or mental suffering, that it is most difficult to publish them in all their details. Three cases which I have witnessed during the last year have made a great impression upon me. A lady not far from my residence, the unfortunate subject of alcoholism, and having considerable weakness of the legs, was reduced at length to the utmost stage of prostration by want of food and constant retching. A little brandy was put from time to time between her lips, in order, as was hoped, to eke out her existence a little longer. The end apparently approaching, straw was laid down in front of the house, and her children were sent for in order to take a final farewell, when, after repeated urging on my part, all stimulus was suspended. The sickness soon ceased; the blood became gradually depurated; after a few hours a little food was taken; and in a fortnight's time, this lady was sitting at the dinner-table with her family. I can say nothing, of course, about the likelihood of relapse, as this opens up another subject. But lately, also, I have been seeing a tradesman and his wife, with Mr. Joyce of Notting-hill, both of whom were addicted to intemperate habits, and both, most remarkably, had almost complete paraplegia. We prevailed on the husband to relinquish his drink, and he began at once to improve; but we had no influence on the female, who was approaching the state of delirium tremens. In another case, that of a medical man, who,

after drinking hard, became so ill that he took to his bed, had epileptiform attacks, ate nothing, and was constantly retching, his wife standing over him administering brandy and champagne from time to time to keep him alive a little longer, I succeeded, after several attempts, in inducing his wife and two medical attendants to stop every drop of alcohol. When this was done the patient soon cried out for drink; but, after imploring in vain for some time, he was violently sick, and then sank into a sound sleep. Upon waking he took a little beef-tea, in a few hours ate some solid food, and within a week was back again in his practice.

I do not wish to discuss in this place the merits of alcohol as a remedy, as it is one I constantly use with the utmost advantage; but simply to state my experience that I have never seen the slightest harm accrue from the sudden deprivation of the accustomed stimulus, but, on the other hand, have had the satisfaction of knowing that I have saved fellow-creatures' lives as certainly as if I had plucked them drowning out of the water. I cannot conceive what the objection is to the withdrawal of the alcohol under the circumstances I mention, any more than I can conceive why, when the system is saturated with poison, and the patient dying from the effects of it, such a gross burlesque on the homœpathic doctrine should be practised as to continue administering it.

The purport of this paper is more especially to draw attention to the fact of paralysis, and more particularly spinal paralysis, occurring as a result of alcoholism; and therefore that when a medical man is called in to see a case of this kind, he should remember intemperance in drinking as a possible cause, just as he would if he found an enlarged liver.

If the affection should turn out to be in any way peculiar in its pathology it will certainly deserve a distinct appellation; but even should the morbid changes in the cord, together with the resulting symptoms, resemble what is seen in other forms of paralysis, I would still recommend the adoption of such a term as alcoholic paralysis as significant of its cause, for we are warranted in so doing by the use of the expression puerperal, syphilitic, or uræmic epilepsy (eclampsia) in reference to the origin of the fits when arising under special circumstances.—*Lancet, March* 9, 1872, *p.* 320.

---

## 29.—TRAUMATIC NEUROMA, CAUSING PARALYSIS: RAPID AND COMPLETE RESTORATION OF THE FUNCTION OF THE NERVE AFTER REMOVAL OF THE TUMOUR.

By T. R. Jessop, Esq., Surgeon to the Leeds General Infirmary.

The following case affords an interesting example of the rapidity with which, under favourable circumstances, a divided

nerve will reunite, even when there has been considerable loss of subtance. The ready manner in which the nerve resumed its functions, after these had been in complete abeyance during nine years, gives to the case additional physiological value.

Emma Ross, aged 19, residing at Bramley, was admitted into the Leeds General Infirmary on Febuary 9th, 1871. When ten years old, she cut the inner side of her wrist with a glass bottle. This was followed immediately by loss of sensation on the inner side of the hand, and by diminished power in the entire hand. When, after many weeks, the wound had at length healed, the resulting scar remained painful and tender. At times, the pain was so great as entirely to prevent sleep, and was not always confined to the seat of injury, but occasionally extended up the forearm and arm, and frequently downwards through the hand and two inner fingers. During the few months immediately preceding her application to me, the pain had increased in intensity.

The cicatrix was situated about two inches above the right wrist, and extended across the half of the anterior surface of the forearm; it was hard and very painful on pressure. The hand, as compared with the left, was thin and wasted. The muscles which should have formed the balls of the little finger and thumb had disappeared; and between the metacarpal bones, both back and front, were deep hollows. Over the inner third of the hand, the whole of the little and the inner half of the ring fingers, the skin had completely lost its sensibility, and sensation was imperfect over the radial half of the ring finger. The movements of the hand were so feeble that the limb was of little use.

On the day of admission, Feb. 9th, the ulnar nerve in the neighbourhood of the cicatrix was laid bare to the extent of about an inch, by an incision along the inner border of the forearm. Just where it traversed the cicatrix, the nerve presented a globular enlargement of the size of a pea. This, together with about half an inch of the involved nerve in its entire thickness, was removed, when the divided ends immediately retracted. In order to approximate the cut ends of the nerve, it was found necessary to flex the hand upon the forearm. Whilst the hand was so held, the ends of the nerve were brought accurately together by means a fine carbolised silk suture, and so retained. The operation was performed, and the wound dressed, in accordance with the strictest antiseptic principles.

For three days after the operation, there was considerable pain along the course of the ulnar nerve, as high as the middle of the upper arm, which was relieved by repeated subcutaneous injections of morphia. The wound healed by first intention.

On Feb. 17th, there was the first indication of returning sensation on the ulnar side of the little finger; and the patient now complained of severe pains shooting from the tips of the little and ring fingers to the wound. From this time the pain gradually diminished, whilst sensation extended and increased.

On Feb. 24th, sensation seemed to have fully returned everywhere except at the extreme tips of the two fingers, and the pain had all but ceased. The hand was now released from its flexed position, and gentle passive motion of the wrist commenced.

On March 11th, as there was inability fully to extend the wrist, the limb was bound down upon a straight splint. On March 20th, she was made an out-patient, and ordered to attend twice a week for galvanism of the muscles of the hand. On April 20th, she could use the hand well; had no pain; could feel perfectly everywhere, except at the tip of the little finger, which, on examination, was found to be covered with a thick layer of cuticle; and the muscles of the hand showed signs of returning development. She can now (November) use the hand for all purposes; the muscles have to a great extent re-formed, but the right hand is still somewhat thinner than the left, and sensation is still wanting at the tip of the little finger, though it is everywhere else perfect.—*Brit. Med. Jour., Dec.* 2, 1872, *p.* 640.

---

## 30.—THREE CASES OF NEURALGIA CURED BY THE USE OF THE CONSTANT GALVANIC CURRENT.

By Joseph Stead, Esq., Manchester.

*Case* 1.—The first occurred to a well-known merchant in Manchester. He was forty years old, and came to me on the 5th of June last, with intense frontal neuralgia, which had lasted a fortnight. It was most severe in the supraorbital branches of the frontal nerve. Five minutes' gentle application of the continuous current by means of small wet sponges attached to small conical conductors sufficed to cure him of his torment on the spot, and it never returned. "I wish," he said the next day to me, "I had known you a fortnight ago. I should have had a very different Whit-week to what I have had!"

*Case* 2.—The second case I shall mention occurred in a young lady, a governess with a leading merchant's family in this neighbourhood. She had intense neuralgia of the left side of the face, which she attributed to the partial extraction of a tooth ten days before. I applied the current as in the last case but with slender hopes of relieving her. One application, however, completely freed her from pain for a fortnight; and then

she returned with it again, when a similar application effectually removed it, and there has been no return.

*Case* 3.—My third case occurred on the 4th of this present October, to a valued female servant of an old patient of mine. She was a girl of about twenty-four years of age, and had been suffering from facial neuralgia of both sides for three months. For two nights previously to coming to me, she had been compelled to get up from bed and come down stairs and spend the night awake, and in great agony. Five minutes' application cured one side of the face, and a second five minutes' application cured the other, and then, to my surprise, "tired Nature's sweet restorer" overwhelmed the poor girl, and she fell fast asleep in her chair at my house. And here, again, there has been not the slightest attempt at return of pain.

These three cases have been selected from the records of my practice in the use of electrics during the last two years in the cure of disease to illustrate the truly marvellous power which the constant galvanic current has over pain. The battery used in all three was Weiss's, sometimes known as Foveaux's. I used about eight cells of this splendid battery, with very small sponges (about as large as would fill the end of a thimble) soaked in warm water, and fixed, as I have just said, to those small conical electrodes which are used for the localisation of the current in paralysis of the interossei and lumbricales muscles. I applied them to the part where the pain was, keeping them about one inch apart from each other. I moved them about, but did not *remove* them, or either of them, from the skin for about two minutes. I then rested for one minute, and applied them again for two minutes more. If the pain had not been removed I should have gone on for another five minutes. But as soon as the pain ceases in a case of neuralgia I make it a rule immediately to discontinue the application. In each of these cases I directed the patient to come daily for a repetition of the operation. But, happily, there was no need for this.—*Medical Press and Circular, Nov.* 1, 1871, *p.* 384.

---

## 31.—THE PHYSOSTIGMA VENENOSUM (CALABAR BEAN) IN EPILEPSY.

By Dr. S. W. D. Williams, Medical Superintendent of the Sussex Lunatic Asylum, Hayward's Heath.

[The use of Calabar bean in epilepsy was suggested to Dr. Williams by reading Dr. Fraser's paper on its use in tetanus and chorea (*Retrospect*, vol. lviii, p. 59). Dr. Fraser shows that it "directly and powerfully diminishes the reflex activity of the cord." It appears to act chiefly on the motor tract of

the cord and on the motor nerves, destroying their conductivity. The great remedies for epilepsy, bromides of ammonium and potassium, also destroy reflex activity of the cord, but by acting chiefly on the sensory tract. Animals poisoned with it retain voluntary movement, whilst reflex movement is destroyed. Calabar bean has been successfully used in tetanus, and it will be seen from the above that *a priori* we might expect it to be of service in epilepsy.]

My experiments were conducted as follows. I chose twelve ordinary epileptics in the Sussex County Asylum, and noted the number of fits daily for six months; I then had them all weighed, and put them on 1 gr. of the bean each twice daily. This I continued for six months, weighing them every month, and increasing the dose of the drug by $\frac{1}{2}$ gr., until the sixth month, when they took $3\frac{1}{2}$ gr. twice daily. I then left off all the medicine, and registered the number of fits for another six months. I also, during the months the patients were taking the drug, paid special attention to the pulse and temperature, and any unusual phenomena.

For convenience of reference I have arranged the results of my observations in a tabular form.

From one table it will be seen that in six of the cases under treatment there was a very considerable decrease in the number of fits during the six months they were taking the bean, as compared with the six months previously; and, what is still more remarkable, that when the bean was omitted the fits, without exception, began to increase again in every case. This was very marked in some of the cases.

In another table we have recorded the weight of each patient taken on the first day of every month whilst they were taking the Calabar bean. It will be seen that seven increased in weight, four lost, and one was stationary.

The third table compares the results of the two previous ones. From it we see that out of six patients whose fits were lessened, three lost weight; whereas only one lost weight amongst those who were not benefited by the drug. But this fact, if worth anything, seems contradicted by the case of C. A., who gained 10lbs., and yet is the very patient whose number of fits experienced the greatest diminution in numbers. And again, A. B., whose fits increased from 6 to 27 when taking the drug, gained 7lbs. Therefore I fear that the only fact these observations as to weight prove is that, as a rule, the Calabar bean given in doses up to 7 grs. a day does not interfere with the nutrition processes, or, at all events, not materially.

It has already been pointed out that the increase in the number of fits of the six who were not benefited during the use of the drug was much less than the increase of the number of

those improved; so that the improvement in the one class of cases considerably counterbalances the deterioration in the other.

Such being the case, it was reasonable to suppose that, assuming the drug to be the cause of the improvement, *cæteris paribus* we should find the cases in which this improvement was manifested to be of one peculiar class, and to differ either in their etiology or pathology, or in the phenomena accompanying the epilepsy, or the intensity of the prodromata, from the other cases, in which there was no improvement. But I am bound to record that, notwithstanding most careful examination and a thorough knowledge of each case, gained by years of intimate watching, no such distinction could be marked.

If the result of the administration of Calabar bean is to deaden reflex action, I surmised that if I could demonstrate the epilepsy in the improved cases to be due to eccentric irritation, we might here have the explanation of the improvement, and I directed my attention specially to this point, searching carefully in each case for indigestion, masturbation, worms, catamenial irregularity, &c., and for prodromata, but without result. I cannot but think, however, that these are the cases in which the bean is likely to be of most service. Unfortunately they are not often met with in asylums, so that I have had no chance of trying.

Another point to which I directed my attention was the fact as proved by Dr. Fraser, that the action of the bean tends to expand the arterial walls. Now it is a well-known fact that in some cases of epilepsy a remarkable pallor often accompanies, and in some cases precedes, the period of tonic spasm of the seizure, together with a lessening of the volume of the pulse. I could not, however, find that this pallor was, as a rule, confined to those cases benefited by the drug; neither could I prove that the length of time it lasted, or its intensity, was diminished. Indeed it was an ordinary accompaniment of the fits alike in some of the unimproved as the improved. Epilepsy is believed by some to be due, as Dr. Needham reminds us, to primary irritation of the sympathetic, causing the vaso-motor nerves to increase arterial tension, and so producing sudden cerebral anæmia; and Dr. Fraser's experiments prove that the effect produced in the heart by the bean is not through the vagi nerves, but through the cardiac ganglia; so that the bean may relieve epilepsy by its power of affecting the sympathetic, and thus reducing the tendency to the above-mentioned arterial tensions.

If in truth the Calabar bean has any influence on epilepsy, I cannot but think that its power must lie in the fact conclusively shown by Dr. Fraser, and which my own observations abundantly bear out—that even in moderate doses it causes

a decrease in the volume and frequency of the pulse, acting of course through the heart.

In this property it resembles the bromide of potassium.—*Practitioner, Feb.* 1872, *p.* 75.

---

## 32.—HYPODERMIC INJECTION OF NICOTINE IN CASES OF TETANUS.

By Dr. E. M. Wuth, Surgeon to the Springsure Hospital, Queensland.

John M, æt. 21, had never been ill before, save some three months previously, when he received a scalp-wound, by a fall from his horse against some sharp stones. There was loss of memory after the fall, but in a fortnight the patient had quite recovered and was able to follow his usual occupation as a shearer. Of late he had been employed at the Washpool, whereby he was obliged to stand for hours up to his hips in water. On my arrival I found him in the following condition: he was laying on a stretcher and resting on his back, with his knees drawn towards the face. The jaw was fixed and the angle of the mouth drawn backwards. The pupils were slightly dilated, the eyeballs fixed, the neck stiff, and all the muscles, especially those of the abdomen, rigid. When the spasms were at their height, the abdominal muscles pressed with such severity on the bowels that flatus was audibly discharged by the rectum, and a foam appeared between the teeth. This stage lasted about a quarter of an hour, after which a partial relaxation took place, during which time the patient could separate his jaws to the extent of about half an inch. The bowels being confined, a draught was administered, when it was found that the muscles of deglutition were affected with tetanus. Castor oil with soap water was therefore given per anum, and a copious motion soon followed. The bladder being distended with urine, this was drawn off by the catheter. The patient now with difficulty moved his arms, and pointed to the lower part of the sternum and lower ribs as the seat of pain. Though sensible, he could not speak. I was told by his friends that he had been in this condition for the previous 48 hours. There was no sleep and no ability to take food, and even drinks could not pass beyond the mouth; nourishing enemata were therefore employed. As to treatment, the maxim, "Contraria contrariis," has never been worthier in my eyes than as to the choice of remedies for tetanus. I mean, that for its complete cure, nothing can be more endowed with success than the production of a state in the system exactly opposite to that by which tetanus is caused. From all the remedies tried, I thought chloroform deserved here the first rank, and it was administered therefore

by me for three consecutive days, and with temporary benefit. However, on the fourth day of my treatment, terrible opisthotonos recurred, and the patient was so weakened and the tetanic symptoms had so gained command over him, that it led me to bring H. Hobart's hopes into realization, by injecting half a drachm of a solution of one drop of pure nicotine in an ounce of pure water, into the cellular tissue of the thigh. Though immediately the pulse sank to 60, the temperature showed little deviation. All the muscles, except those of the neck, suddenly relaxed, and presently the patient began to perspire. He fell into a sound sleep for four hours, after which he had fully recovered the use of the organs of speech. His first question was: "What has the doctor done with my thigh? I feel all benumbed!" He then partook of some wine and water, and an egg beaten up in milk. The injection was used at twelve o'clock at night. The patient awoke about four o'clock next morning. One hour later, a pill of one grain of extract. nicotianæ aquos. (Ph. Hess.) was administered to him. He again awoke drowsy at nine o'clock a.m. Although his body felt very sore, the tetanic symptoms had ceased, and from this time his bowels and bladder acted regularly. A pill of the same extract was given for three nights following, after which the patient felt, and has been since, as well as ever.

Haughton and O'Reilly (Medical Times and Gazette, June 12th 1858) highly extolled the antidotic qualities of nicotine and infusion of tobacco in cases of poisoning by strychnine, and considering the physiological analogy of the effects of the latter poison with the symptoms of tetanus, I may urge upon my professional brethren the rationale of the treatment.—*Australian Medical Journal, Sept.* 1871, *p.* 264.

---

## 33.—USE OF NUX VOMICA IN CERTAIN NEUROSES OF ORGANIC LIFE.

M. Brugnoli has employed nux vomica successfully in the nervous movements of pregnancy, gastralgia, dyspepsia, hypochondriasis, nervous palpitations of the heart, nervous and periodic cough, asthma, and finally in albuminuria. This remedy acts either on the pneumogastric, or on the great sympathetic, or on the spinal cord. He records a case of a lady affected with severe cough recurring every evening and lasting throughout the night, who was cured in two days by the use of nux vomica. Another patient was affected every evening with violent cough accompanied by catarrhal expectoration, and was also cured in two days by the use of the alcoholic extract of nux vomica mixed with the extract of gentian. Cough may always be allayed by this means, whether it be caused by bron-

chitis, by pneumonia, by pulmonary phthisis, or by emphysema. It proves a useful remedy also in cases of cardiac pulsations, and in irregular or too frequent action of the heart. In albuminuria, M. Brugnoli thinks the administration of nux vomica has retarded its progress to some extent, especially in cases of scarlatinal albuminuria.—*Practitioner, Feb.* 1872, *p.* 118.

## DISEASES OF THE ORGANS OF CIRCULATION.

### 34.—THE TREATMENT OF HEART DISEASE.

By Dr. J. Milner Fothergill.

[In the treatment of heart affections the first thing to be considered is the removal of the cause. This is of course chiefly of moment in cases which have not advanced far, and are consequently capable of repair. The *causes* of cardiac disease are]

Excessive exertion, Bright's disease, imperfect nutrition, nervous exhaustion, pericarditis, and last, not least, endocarditis, with its consequent valvular lesions, for which there is no repair, and only more or less imperfect compensation, of varying permanency, to be hoped for. To this list must be added syphilis, which is not only a cause of congenital malformation, but affects the muscular structure of the heart itself, as one of its ulterior actions.

Taking them in the above order, we come now to consider the effect of strain on the heart, as the result of violent muscular exertion, *pur et simple*, without any want of nutrition, or other failure, in the heart itself. This is not associated with an active or industrious life, with exertion prolonged as a daily task, a life-long duty, but with violent temporary exertion, the consequence of labour in some forms of industry, or in voluntary effort, as in the gymnast. This cause is seen largely in action in hammermen, who have to put forth all their strength and endurance during the brief period of time that heated iron remains sufficiently hot to be worked. During the process of heating the man is resting, true, but the vascular excitement and heaving respiration tell how severe is the strain to which the viscera have been subjected. Let us consider what the *modus operandi* is here. Firstly, there is violent action of the muscles of the upper limbs, which compress the arteries, and even act more or less across them (the musculo-cardiac function of Wardrop), and form a decided obstruction to the flow of blood from the left ventricle. The same action of the muscles, on the other hand, tends to drive the venous blood towards the heart. The blood is thus collected in the venous centres—viz., the venæ cavæ, right heart, and pulmonic circulation. The

chest is also more or less fixed, as having points from which the muscles work. When each "heat" is over, on examination, the "striker" will be found panting, with violent action of his heart, and bounding pulse, gradually subsiding into a balance of the arterial and venous circulations, until the next effort, which is not far distant.

It is obvious that here there is, of necessity, a strain put on all parts, from the obstructed lesser arteries backwards to the aorta, left ventricle, and the truly venous parts, the pulmonary vessels, and right heart. The morbid results manifest themselves in various parts. For instance, in dilatation of the aorta, with atheroma in the distended endarterium; in thickening and distorting the aortic valves, driven together with excessive force; in yielding of the left ventricle; and inevitably in dilatation of the pulmonary artery, with dilatation and hypertrophy of the right ventricle. The last is almost unavoidable, and tends to perpetuate the mischief inaugurated in the left ventricle, thus—the enlarged and altogether bigger right ventricle sends a greater mass of blood through into the left side, and thus the left side is filled under greater pressure, a well-known cause of dilatation of the ventricle. Its contraction, in turn, throws a larger volume of blood into the aorta, and keeps up the condition of over-distension. This excessive aortic distension leads to a fuller supply of blood to the coronary arteries, and full nutrition of the heart. For a time this condition is maintained, but sooner or later the action changes into some distinctly morbid condition. The distended aorta may be ruptured by a blow, and a true aneurism formed by cracking the brittle inner coat; this distension may go on to atheroma and its consequences; the aortic valves may become diseased and imperfect, or the muscular walls may yield. The disease of the aortic valves has a curious effect on the muscular walls. Hypertrophy, and that to an unusual extent, supervenes, but it is not permanent. Mauriac has most ingeniously shown, that when the aortic valves become imperfect, the blood column in the aorta, no longer perfectly arrested in its backward course by the semilunar valves, does not, on the arterial recoil, perfectly fill the coronary arteries, and sooner or later the hypertrophied walls become degenerated from imperfect nutrition. The changes in the arterial system act on the parts behind them; but the venous system not unfrequently is itself the first to yield. Thus, from the pulmonic congestion comes tuberculosis, emphysema, &c.; from yielding of the right ventricle we have dyspnœa on slight exertion, palpitation, and true cardiac asthma. This condition is common among hammermen, colliers—especially when working in constrained positions, which can only be maintained for limited periods, which, how-

ever, test their indurance—among gymnasts, &c. When, then, any of the earlier symptoms of heart failure, or failure in the other organs, manifest themselves, it is only too obvious that the form of labour which evokes the failure must be abandoned; for the causes which could induce disease in the healthy heart must have a sadly deleterious action when that organ is commencing to yield, and the various forms of compensation to become imperfect. No measures, however successful in affording relief to symptoms, can be expected to endow these organs with the virtue to resist the stress with impunity. By rest, where practicable, and a change of labour—some new occupation not necessitating violent effort, where labour in some form is unavoidable—the changes in the muscular walls may be greatly modified (in proof of this can be adduced the passing away of the hypertrophy of the left ventricle of pregnancy on parturition); and a return to the normal state more or less completely attained. But if the muscular walls have grown equally with the demand on them, and the changes have commenced in the arteries, nothing known in the present state of medicine can in any way restore them. Here the removal of the cause can alone promise anything, or hold out the hope of a prolonged existence.

[Considerable difference of opinion has existed as to the relation of cardiac to renal disease. The opinions of Dr. George Johnson, however, appear the most correct and satisfactory. He shows that in kidney diseases there is]

An hypertrophy of the muscular coat of the arterioles generally, and evolved this theory. The walls of the arterioles are largely muscular, and have a "stop-cock" action in altering the calibre of the arterioles, and thus permitting a larger or smaller bulk of blood to pass to the various tissues on the arterial recoil. The retained urine salts in the blood poison it, and the arterioles contract to prevent the entrance of poisoned blood into the tissues. This constitutes an obstruction to the blood current, and changes in the left ventricle are inaugurated.

The *treatment*, by removal of causation, of the cardiac troubles consequent on kidney mischief, which will be found most effectual, will be the removal of the accumulation of the products of histolysis, since treatment of the kidney itself is impossible.

For the achievement of this we possess two totally different means—either action on the kidney, such of it as remains useful, or the bringing of other organs into increased action; usually both combined will be found most effectual. Thus, an active purgative, by exciting the bowels, will aid in the elimination of the offending material; diaphoresis is an excellent form of

wielding this power of other organs to aid in the elimination of material somewhat special to another organ. The most persistent form which the products of retrograde tissue metamorphosis assume is that of uric acid; and experience has told me that potash and buchu, with colchicum, better relieve palpitation and the other evidences of heart disturbance, in Bright's disease, than did digitalis, belladonna, &c., without this truly specific treatment. The retained uric acid, which can only exist in the alkaline blood, in union with some base, unites with the potash, and in this highly soluble condition passes readily out through the kidneys by dialysis. The action of colchicum and buchu is to increase the elimination of the urine salts by acting on the renal secreting cells, and the consequence of this treatment is a decided increase in the specific gravity of the urine, not attained by diminution of its bulk. This treatment of the heart trouble, consequent on renal impairment, by removing the excess of waste material in the blood, is a most important point in the checking, and, to some degree, arresting the morbid processes already instituted. There is no doubt that the constant presence of uric acid in the blood still further aggravates the kidney mischief already existing. The question of the retention of urea in the blood in acute congestion of the kidney and uræmia, does not bear on the subject under discussion here.

No treatment, however, should be persisted in until anæmia be produced. Defective nutrition is one of the great primary sources of heart failure, and as a secondary factor it very commonly hastens the setting in of loss of that compensatory hypertrophy, which has hitherto ameliorated the mischief done. The tendency of Bright's disease is towards imperfect nutrition, and when this is threatening the treatment must be accompanied, or followed, by the administration of iron. For this purpose the potassio-tartrate is eminently suited, and goes well with the bicarbonate of potash. The effect of blood-poisons of all kinds is decidedly to check blood-formation, as for instance, Bright's disease, syphilis, malarial poisons, lead, &c., and their elimination by their respective specifics, potash, mercury, quinine, iodide of potassium, &c., is the first step to be attended to in the treatment of the affections to which they give rise. In all these conditions the addition of iron, to aid in the formation of new blood, is of great service. In cases of heart failure all conditions of anæmia should be carefully examined and submitted to the treatment adapted to each. Thus, leucorrhœa and menorrhagia frequently cause the anæmia which leads to imperfect nutrition and consequent heart failure.

Pretty much the same effect upon the heart as is produced by anæmia, is produced by nervous exhaustion, only here the

action is more special upon the heart. The effect of nervous exhaustion is largely located in the organic nervous system, and the viscera supplied from it. The effect of this nervous exhaustion is to impair the sensitiveness of the cardiac ganglia, upon which depends cardiac contraction. The sensation of distension of the muscular fibres is recorded by the cardiac ganglia, and contraction follows, as in all hollow or spherical masses of muscular fibre. When, then, from impairment of sensibility a condition of over-distension results, there follows imperfect power of contraction, and thus dilatation is inaugurated. This imperfect contraction is thus productive of dilatation and other consequent troubles. The ventricle not thoroughly emptied is already, on diastole, partially full, it is soon full to over-distension from the pouring in of blood from the gorged auricle and veins behind : imperfectly emptied, it is soon over-full, and frequent imperfect contractions take the place of normal contractions. This condition not only tends to perpetuate itself, but sooner or later to lead to degeneration of tissue. A small bulk of blood only is thrown at each ventricular systole into the aorta; it is imperfectly distended, and consequently its recoil is diminished; this aortic recoil is the propelling power for the coronary arteries; the lessened flow diminishes the nutrition of the heart-walls, and consequently degeneration results. At the same time, too, every additional contraction in the minute diminishes the heart's period of rest. All causes, then, which increase the rapidity of the pulse, and especially when this increase in frequency is accompanied by loss of power in each pulsation, should be avoided, as excitement, anxiety in speculation, excessive drinking, the denial of sleep, especially in the horizontal posture, tobacco-smoking, and, certainly not to be omitted, the excessive use of tea. The effect of agents which lead to nervous exhaustion and increased frequency of heart contraction, with loss of quality, is strictly analogous in effect to imperfect nutrition or anæmia. The complete or partial removal, where complete removal is not feasible, of these causes is imperatively demanded in the treatment of the heart failure which results from them.

[Dr. Fothergill then passes on to consider our means of acting on the heart itself.]

Can we increase and diminish the completeness and power of the ventricular contractions ? is a question which can now be answered in the affirmative. There do exist agents which positively stimulate the heart-walls into increased contraction. Thus the conditions of the heart-walls as regards distension is brought within the reach of remedial agents administered internally. The effect of such an agent in the condition described immediately above, is to cause the ventricle to contract more

perfectly; and thus it is not only relieved from the danger of paralysis from over-distension, but aided to recover itself. No sedative to the heart is needed here betwixt its distension and the risk of paralysis from the carbonic acid of the highly venous blood within it: it is only already too powerless; but some agent which will enable it to contract more efficiently. That we are in possession of such agents is certain. The chief one in use is digitalis.

Physiological experiment demonstrates what clinical observation had long expected, that the effect of digitalis is to produce more perfect action of the ventricles. The condition of complete paralysis by aconite, identical with that of the over-distended heart, was quickly relieved by the administration of digitalis; and not only so, but that previously paralysed heart, in extreme distension, can be brought to a standstill in the opposite condition of complete contraction, if enough of digitalis be administered. In practice, then, digitalis can be administered with excellent effect in this condition of over-distension of the right ventricle, and has been so used. In all conditions where the muscular walls of the heart are unequal to the demand on them, whether from valvular disease or other cause, digitalis can be given, not only for a time, but continuously and uninterruptedly for years, with excellent effect. The continuous and persistent administration of digitalis in cardiac inability not only secures the immediate effects of improved ventricular contraction, but has an ulterior effect upon the heart itself; and oftentimes the heart, getting further and further distended and enfeebled, sending less and less blood into the aorta at every systole, and thus eliciting less aortic distension and consequent recoil, and with that diminished recoil an impaired coronary circulation and supply of blood to itself, thus ensuring more complete muscular failure, is, by digitalis, enabled to contract more perfectly, to elicit better aortic distension and recoil, better supply of blood to itself, and improved nutrition; while every contraction fewer in the minute is adding to its period of rest. Thus, not only can a downward progress be arrested, but even a career of improvement inaugurated, which in simple dilatation or distension may result in perfect restoration to health. Where this cannot be accomplished, not only may life be made more bearable, but even be prolonged; and the question of the importance of prolonged life in individuals is one the importance of which has been pointed out by Quetelet (Sur l'Homme); while that importance itself is materially enhanced by combining with it a power for more or less labour, corporeal or mental; and that that is within the clear limits of possibilities is no therapeutic fiction, whatever doubts may be cast by some on the possibility of perfect restoration to health.

Even in the worst cases patients cling to existence, and something may be done to relieve the suffering which results from heart failure by thus acting on the muscular walls by digitalis.

Along with this direct treatment of the heart itself may, and often must, be combined diminished call for exertion on its part; rest is imperative for a time, or more or less permanently, and the improvement in the coronary circulation must be supplemented by improvement in the blood itself. Iron, cod-liver oil, arsenic, and the other means of improving blood-formation, and from it again improved nutrition, are aided by the vegetable tonics strychnine, quinine, &c., which improve the digestive powers of the stomach. As to iron, Fuller says that the continuous administration of iron for three or four years has resulted in the removal of every physical sign and rational symptom of dilatation. Thus the heart itself has a power of recovery when furnished merely with an improved blood-supply without the aid of means acting directly upon itself. In practice, the combination of different measures may be indicated or demanded, and some of the formulæ most used by the writer were these—

1. ℞. Pot. bicarb. gr. v. or x.; ferri pot. tart. gr. v.; tinct. digital. ♏ x.; inf. calumb. ℥1, ter in die.
2. ℞. Tinct. digital. ♏ x.; sp. chloroformi, ♏ xxv.; inf. buchu, ℥1, ter in die.
3. ℞. Tinct. digital. ♏ x.; liq. strychnia, ♏ v.; tinct. fer. mur. ♏ v. or x.; aquæ or inf. quass. ℥1, ter in die.
4. ℞. Tinct. digital. ♏ x.; sp. æth. nit. ♏ xxv.; tinct. sem. colch. ♏ x.; inf. buchu, ℥1, ter in die.
5. ℞. Pulv. digital. gr. $\frac{1}{2}$.; pulv. ferri sulph. exsic. gr. $\frac{1}{4}$.; pulv. pip. nig. gr. 1.; ext. gent. or pil. al. et. myrrh, q. s. 1. bis. in die.
6. ℞. Tinct. belladonnæ, ♏ xx.; tinct. nucis vomicæ, ♏ x.; mist. camphoræ, ℥1, ter in die.

These different formulæ may each be useful, and the second is especially agreeable in simple cardiac debility: the first is better when washed down with a tumblerful of water, and with the pills a patient may go away on a visit very comfortably. Thus by digitalis we can, more or less, according to the nature of the case, and the integrity of the heart-walls, act upon a heart whose contractions are becoming incomplete. This is the centre point of action upon the heart directly; for we must all be only too painfully aware that any action on deceased valves is beyond the reach of remedies, in this age as yet. But whether the origin be valvular failure or other cause, as long as the heart can contract completely a better state of matters is maintained than when the heart-walls fail. It is this failure of power, this inability to contract completely on the part of the ventri-

cles, which is the thing we have to dread as being the end, or at any rate the beginning of the end, of the case. It may not be out of place here to add that in electricity we possess a power that may be of great service in the treatment of heart failure. The effect of a feeble electric current is to increase the force and lessen the frequency of the heart's action: as the writer has observed with a small Stohrer's battery, one pole placed over the heart and the other to the nape of the neck.

*Rational treatment of Sequelæ.*—The third and last division of the treatment of heart affections is only of less importance than the preceding divisions, because it does not so much prevent as alleviate evils. Still, when from inability in the centre of the circulation we have decidedly venous congestion, and from it, again, a sort of re-balancing of the system arterial and venous, we have the different viscera in a state of asthenic congestion, a condition unfavourable to the performances of function. Thus, we have gastric catarrh, congestion of the liver, diminished flow of urine, and tendency to disease of the kidney, as the consequence and not the cause of heart disease, in females uterine discharges menorrhagic or leucorrhœal; so we have effusion through the lining membrane of the air-passages, and a flow of fluid, chronic bronchitis or bronchorrhœa, œdema of the lungs, &c., &c., and finally general anasarca. Now, it is obvious that where we hope not only to relieve matters, but to inaugurate improvement, we must relieve the viscera from a condition unfavourable to improved nutrition. For this purpose we can use purgatives, diuretics, diaphoretics, and even expectorants; all agents affecting the secretory and excretory power of the different organs.

Purgatives, and more especially drastic cathartics, have been long resorted to with the intention of calling out the action of the bowel; and the different effect of two drachms of pulv. jalap. co., with a grain of gamboge in it, on a patient with advanced heart disease, to its effect on a healthy person, speaks volumes. The latter would feel decidedly depressed, but the sufferer from heart disease feels enlivened, sprightly, and altogether improved. Four or five copious watery evacuations have relieved his venous congestion, relieved his distended right ventricle, and improved his pulse, while the relief to the cerebral congestion manifests itself in his improved spirits. This method of acting on the viscera in the congestion of heart failure is deservedly a favourite one; and for the purpose of exciting free catharsis the usual means are pulv. jalap. co., pulv. scam. co., elaterium, and gamboge. Gamboge rubbed up with bitartrate of potash and black pepper, or other carminative, is a capital cathartic. All may have to be used in turn, as one form soon loses its efficacy. Chronic diarrhœa is not in this

condition to be arrested, except by action on the skin, unless it is clearly threatening the patient's existence: it is a natural drain, as much as eczema in hydrops. Purgatives are often thought to have a diuretic effect in heart affections; this is the impression of experience as a fact. Really, the improvement in the circulation is followed by freer circulation through the kidneys and better arterial tension, and consequently there is better diuresis of water.

Diuretics have not only always been resorted to in the treatment of dropsy, which was, until recently, regarded as a disease *per se* and not a sequel, but the improved or unaltered flow of urine has been adopted as a sort of test of the efficacy of the remedial measures resorted to. Long before the experiments of Paff, or Winogradoff, Handfield Jones, and Brunton, on arterial tension, popular observation had learnt that an increased flow of urine indicated an improvement in the patient's immediate condition in dropsy; and it is therefore, in the agents classed as diuretics that we find digitalis, squill, belladonna, &c., which we now know exercise their diuretic action by virtue of the increased arterial tension, the result of more perfect ventricular contraction. The water excretion by the kidneys is a question of hydraulics—a purely mechanical result of the pressure of the blood on the thin-walled glomeruli of the Malpighian bodies. When the arterial blood is driven in but feebly, and the venous is almost stagnant, we find the bulk of urine very low indeed: but its sp. gr. may be very high, for the bulk of urine and the elimination of solid excreta by the renal secreting cells bear no necessary relation to each other. No doubt, too, certain other agents act as diuretics, as juniper, turpentine, potash nitrate, nitric æther, &c., by their being direct stimulants to the kidneys, and thus inducing an increased flow of blood to these organs. But it is in the first division we find our agents which are the best diuretics in heart failure, and what position scoparium should hold it is difficult to say from want of observation and experiments, but it will probably be found among the first named. The other class of diuretics, as colchicum, cantharadine, and buchu, &c., which increase the amount of solids without affecting the bulk of urine, are not to be regarded as diuretics in the treatment of heart failure. Stimulants are diuretics insomuch as they increase the completeness of the ventricular contractions and improve arterial tension.

Diaphoretics are not yet sufficiently adopted as a means of affording relief in dropsy and the sequelæ of heart failure; but it is not only a very effectual, but, in my experience so far, a perfectly safe way of increasing water elimination, especially when the heart failure is accompanied by renal disease. The

sitz-bath is a good form, or any bath which permits the patient to breathe freely; Turkish baths are regarded as not very safe. Another plan, employed by the late Sir James Simpson, has been so useful that it cannot be omitted, and that is, to fill six or eight lemonade bottles with boiling water, cork them well, and draw over each a woollen stocking wrung out of hot water, then pack them round the patient in bed, who in fifteen minutes or so commences to sweat profusely: they can be continued for an hour, or repeated when necessary. This plan can be resorted to with the poorest patients, and when a patient of any class is confined to bed. In obstinate cases, where the skin will not be induced to relax by hot baths, cold packing has been resorted to in order to paralyze the minute vessels of the skin, and thus produce dilatation of them in cases of chronic kidney disease; but I am not acquainted with the details as to whether cases with distinct heart failure were so treated or not.

Dropsy is not only a great cause of suffering to a patient with heart disease, but is regarded by non-professional people as indicating great danger to existence. It is, no doubt, a sign of failing power of serious import; still its occurrence often gives temporary relief. When the distended venous system is relieved by a sudden general œdema, great and immediate relief is experienced, and that may be utilized to improve matters and to induce re-absorption. But usually dropsy commences insidiously, retreats for a longer or shorter time under proper treatment, but usually it is not long in returning, and is more persistent on each reappearance. If, during the interval, the patient has been under good treatment, and yet in spite of it it returns, it is a prognostic sign of the very worst import: no worse sign can exist, and its reappearance calls for the most energetic treatment. Sometimes œdema is very capricious in its seat, and flits about in a curious to-and-fro manner; in one well-remembered case, where the lesion was tricuspid, the oscillations from legs to lungs and back again were very marked. If the patient was found with a less agonized countenance, his legs were tremendously swollen; when he was gasping for breath, his legs were fallen. Dropsy, as a sequel to heart disease, is amenable, more or less, to the measures cited above, in addition to which is the question of puncture. Nature oftentimes establishes a drain of her own; not always with good effect. Puncture, or incision, is, in my personal experience, in most cases worse then useless, only adds a wet bed to the patient's sufferings. One case only can I recall where the benefit was decided and unequivocal. Others have had a less unpleasant experience, but it is in dropsy from heart and kidney disease combined that the benefit of puncture or incision is chiefly seen. In simple

heart failure it is a hazardous proceeding, and sometimes the carbonic acid poisoning, the irregularity of pulse, dyspnœa, &c., commence to increase on the establishment of the drain. In one case this occurred when the serum spirted out of the needle-holes in an arterial jet, and the bed was soon in a perfect puddle, and could not be kept otherwise during the six-and-thirty hours the patient survived. Still the puncture, or incision, is among our armamentaria, and does frequently afford more or less relief; but the incisions may become troublesome sores, or even slough.—*Edin. Med. Journal, March* 1872, *p.* 776.

---

## DISEASES OF THE ORGANS OF RESPIRATION.

---

### 35.—ON THE APPLICATION OF THE DIAPASON TO CLINICAL AUSCULTATION.

By Dr. W. Handsel Griffiths, Assistant Librarian to the Royal College of Surgeons, Ireland.

An extensive study of the science of acoustics with reference to clinical auscultation leads me to suggest, as a valuable adjunct to physical diagnosis, a modified form of the ordinary diapason, or "tuning-fork."

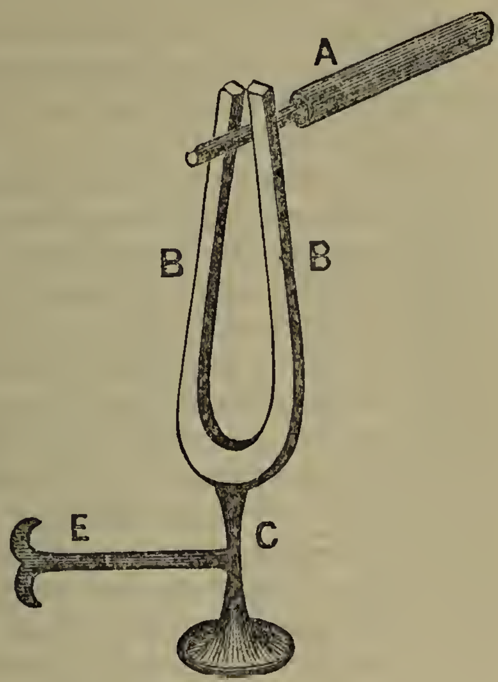
It is a well-known fact that the intensity of sound evolved, or the degree of resonance elicited, by a vibrating diapason, varies with the nature of the substance or surface on which it rests. I have been lately engaged in an experimental inquiry with a view to define the value of this principle as applied to clinical auscultation, and have been much gratified by the result. The form of diapason which I have hitherto used is intelligibly figured in the annexed cut. B B are the "prongs," which, on being suddenly separated by the "sounder," A, vibrate, and so evolve sound; C is the "connector" by which the sound is communicated to the small bell-like "dome" (similar in general size and shape to the small end of the stethoscope); and E is the handle by which the instrument is held in apposition with the surface of the organ or part under examination.

Now, although the variable resonance afforded by this instrument on application to different regions of the body is most remarkable, and beautifully indicative of abnormal condition or altered structure, I conceive that it is possible to invent a more delicate and definite indicator; but it would probably be in accordance with the same principle.

It is not in pulmonary affections only that such an instrument is likely to prove valuable; in almost every abnormal condition of almost every organ the indications afforded by

it are likely to prove of inestimable service in forming a diagnosis.

Now I am not promulgating these suggestions as the result of mature or perfected research, but I have thought it expedient to publish them, crude as they are, in the hope that the attention of abler minds and more acute intellects may be attracted to the subject. The field of observation thus opened is, I believe, a wide one, and likely to repay a careful and protracted investigation. I trust that the co-operation of contemporary physicists and physicians will enable me, ere long, to offer to the profession a standard instrument, constructed on strict acoustic principles, which will afford definite and delicate indications of variable resonance, and which will be reliable as an index of altered structure and organisation.




I am not aware that the idea of applying the diapason, or any similar instrument, to clinical auscultation has hitherto been suggested; but if so, I trust that I may be corrected, and that the credit of priority will be claimed by the originator. At all events I am convinced that the subject, if not novel, is well worthy of revival.—*Lancet, Dec.* 16, 1871, *p.* 848.

---

## 36.—ON THE PATHOLOGICAL AND THERAPEUTICAL RELATIONS OF ASTHMA, ANGINA PECTORIS, AND GASTRALGIA.

By Dr. F. E. Anstie, Senior Assistant-Physician to Westminster Hospital, &c.

[The following is the proposition submitted in this paper:—that asthma, angina pectoris, and gastralgia (or neuralgia of the gastric nerves) are essentially dependent on neurosis of the vagus nerve, which is of central origin, and in a large majority of cases is mainly or entirely to inherited peculiarities of the central nervous system.]

The evidence arranges itself under five sections:

1. Inferences from the known physiological functions of the vagus;

2. Evidence of the interchangeability of asthma, angina, and gastralgia, in the same individual;

3. Evidence of the pathological connection of these neuroses with neuralgia of the fifth nerve;

4. Evidence of the common dependence of asthma, angina, gastralgia, and neuralgia of the fifth, on peculiar inherited neurotic tendencies;

5. Evidence from the similarity of effects produced by certain remedies on all these maladies.

1. As regards the physiology of the vagus, there is no need to deliver a class-room lecture to the present audience. It will be enough to remind you that, besides its other offices, the pneumogastric acts as the great afferent of sensory impressions from the stomach and from the mucous surfaces of the lungs and trachea, and as the main, if not the sole, organ of cardiac sensibility; that is ramifications form an integral part of each of the various nervous plexuses in the cardiac, gastric, and respiratory territories; and that its deep origin is placed within the medulla oblongata, and united by the most intimate connections with the sensory nucleus of the fifth nerve. There are thus *primâ facie* reasons for presuming that the three neuroses, asthma, angina, and gastralgia, would all depend upon irritation of the vagus. As regards asthma, there is I suppose, a pretty general adhesion among the best recent authorities to the theory of irritation of the pneumogastric, which is well expressed by M. Sée of Paris in an article on the Treatment of Asthma in the Practitioner for July, 1869. As regards the pain of angina, and indeed as regards the sources of cardiac sensibility in general, there has been much dispute; but the facts seem to me to point very clearly to the belief that branches of the vagus, reinforced in the cardiac plexus by other spinal branches which come from the cervical and the uppermost dorsal cord-centres, through the medium of the sympathetic ganglia of the neck, fully account not only for heart-sensibility, but also for the curious transmission of pain in angina to the arm, the chest-wall, and the neck. The chief objection to this view has rested upon the belief that the sympathetic branches must also have a share in the production of the pain; but the idea that the sympathetic is capable of expressing sensations of pain seems to me to rest on an exceedingly doubtful basis, and to be supported chiefly by the apparent analogies of the pain of colic, gall-stone, &c., for which pains there is really a much more probable source in branches of spinal nerves. Everything in recent physiological observation seems to suggest that all

the apparent cases of sensation in the sympathetic fibres are really due to branches of spinal nerves that are bound up with them. As regards angina, the only question as to which I feel much doubt is, whether the vagus centre or the cervical and upper dorsal cord-centres just referred to are most prominently concerned in the origination of the disease; but, at any rate, it seems clear that, from the moment that serious symptoms of angina are set up, the vagus centre must be involved, since this must be the intermediate instrument of the reflex disturbances of respiration which occur during the paroxysms. On the whole, I lean strongly to the idea, which will be found expressed in the pathology chapter of my work on Neuralgia, that the irritative process in angina commences in the spinal centres which send branches to the cardiac plexus, and then involves the vagus secondarily, but at a very early period.

2. The second branch of the evidence that connects asthma, angina, and gastralgia, all with the same great nerve, is derived from the singular interchangeability of the three maladies. Kneeland long ago remarked that spasmodic asthma and angina were thus apt to pass the one into the other. For my own part, I can refer to several instances of the connection between all three diseases, and would sum up the general results of what I have seen in the following terms. 1. Severe and protracted asthma nearly always becomes complicated, after a time, with symptoms which may not amount to what would commonly be called angina, but assuredly belong to the same category; and in not a few instances (I have myself seen three) asthma leads directly into declared and unmistakable angina. 2. Asthma is nearly always associated with gastralgia. It is true that the neuralgic character of the pain is very often overlooked, and it is attributed incorrectly to dyspepsia; but I have frequently had occasion to prove the fallacy of this idea, the only ground for which is, that asthmatic patients will not bear putting a large amount of food in the stomach at once. Asthmatic patients are not commonly dyspeptic in the proper sense of the word; and the epigastric pain, from which they almost all suffer more or less, requires to be dealt with as a part of the general nervous affection. 3. The chronic form of angina is, so far as I am aware, almost invariably associated with more or less gastralgia, which, as in the case of asthma, is continually being mistaken for dyspeptic pain, and treated, with very unfortunate consequences, from a gastric instead of a nervous point of view. Like asthmatics, these patients require to be fed in small *meals;* but nothing save harm can come of keeping them on a small total *supply* of nutriment.

3. The next branch of evidence for the essential interconnexion of the three neuroses of which we are speaking is

derived from the way in which they are mixed up and complicated with neuralgia of the fifth nerve—a complication which, if it can be shown to occur with a certain special frequency in these diseases, would go far to trace home the *fons et origo mali* to that portion of the medulla oblongata in which the nuclei of the vagus and the fifth are found in intimate connexion with each other. Upon this point the evidence is, in my experience, exceedingly strong, and may be shortly stated thus: that, whereas neuralgia of the trigeminus is a more or less occasional complication or attendant of other neuralgias generally, it is an almost invariable complication of nervous asthma, nervous angina, and gastralgia. Of course it varies greatly in severity. It may amount, and indeed generally does, to nothing but an attack of migraine every month or six weeks; but there it is, nearly always. And, as regards gastralgia, I have seen numerous instances of its interchangeability with facial neuralgia, of which the following is perhaps the most striking. A watchmaker's assistant, aged 42, applied to me at Westminster Hospital, suffering from intermittent attacks of epigastric pain, which came on specially at times when he was exhausted from want of food, and were uncommonly severe and prostrating. After he had been under my care for ten or twelve days, these pains were abruptly superseded by violent neuralgia in the globe of the right eye and in the branches of the ophthalmic division of the right fifth nerve generally. This neuralgia was not merely severe in itself, but it ran on into secondary iritis, with destruction of the eye for visual purposes. This is the most severe case of trigeminal neuralgia complicating gastralgia that I have seen, but I have witnessed plenty of slighter ones; and on the whole, I believe that true gastralgia, as distinguished from mere dyspeptic stomach-pain, is nearly always attended with a tendency to facial neuralgia in some form and degree.

4. In the next place, the close interconnexion of asthma, angina, and gastralgia, and their common dependence on an affection of the central nervous system, is rendered more probable by the remarkable history of inherited neurosis which the sufferers from them will always be found, upon careful inquiry, to present. This is a branch of investigation which has greatly engaged my attention; and in my work on Neuralgia, the results of a good many such inquiries as to the pedigree of neuralgic patients will be found. But I have not had space for everything in that work, and must probably reserve for a separate publication the remarkable facts which come out respecting the hereditary connexion of asthma and angina pectoris. Respecting gastralgia, it has been, for obvious reasons, impossible to procure evidence of a precise, and reliable character;

but as respects asthma and angina pectoris, I will go so far as to say that I believe there hardly ever existed a sufferer from one of these diseases in whose blood-relations, either of the same or previous generations, one or more examples of the other could not be found by proper inquiry; and occasionally one comes across families which seem to have been quite plague-stricken, as it were, alternately with these two maladies. And it is a singular fact that I have twice known these diseases to be the only nervous maladies that could be traced in a large family of two or three generations.

I may here properly introduce the reply to an expression of surprise which some of my audience may have on their lips, at the frequency which I ascribe to angina pectoris, which, on the contrary, has usually been considered a very rare disease. But I have already had the honour to tell this Association (at the meeting in 1868) that observation had convinced me of the error of that opinion, which arose from neglect or misunderstanding of all but the severest and suddenly fatal cases of angina; whereas there are, in fact, a multitude of cases in which the nervous affection—the cardiac neuralgia—which is the only essential portion of the disease, exists in all shades of severity, down to an exceedingly mild and trivial complaint. And my subsequent experience has stongly confirmed me in this belief, which is also expressed by some of the most careful recent writers.

5. The last, and to me personally the most interesting, link in the chain of evidence which connects together the three neuroses asthma, angina, and gastralgia, is formed by the results of therapeutical experiment. There is one remedy which is supremely effective, where it can be tolerated, in all these three maladies; namely, arsenic. As regards angina, I may say that, since my attention was drawn to arsenic by a remarkable case published by Philipp in 1865, I have had a rather large number of severe cases to treat; but it is within the last twelve months, that I have received the most conclusive proof of the power of this drug over the nervous symptoms of angina. Two examples especially illustrate this, the subjects being respectively aged 65 and 75. Each of these gentlemen was free from recognisable organic heart disease; but there was much probability in both cases that there may have been a certain amount of ossification of the coronary arteries. Be that as it may, in both these cases, arsenic, in five-minim doses of Fowlers's solution three times a day, relieved the attacks very rapidly, and completely removed them in the course of about a fortnight. There had previously been daily paroxysms in each of these cases; and the elder of the two gentlemen was the more struck with the rapidity and completeness of the relief obtained, inasmuch as he was a

medical man, and was, of course, thoroughly well aware of the intractability and the formidable nature of the disease. As regards gastralgia, the efficacy of arsenic was some years ago pointed out by Dr. Leared ; and I have had several opportunities of proving the correctness of his statement. But more especially is it an effective remedy in that form of gastralgia which accompanies asthma. I have for many years been accustomed to the use of arsenic in asthma, and had seen great benefits produced by its tonic effect. I had also noticed, without thinking much about it, that the tendency to gastralgia, and also to facial neuralgia, was at the same time remarkably diminished ; but at present I cannot regard the coincidence as accidental. In regard to the effects of arsenic upon asthma and angina, it seems proper to refer to the facts, long disputed, but now established as true, respecting arsenic-eating in Styria. It has been ascertained beyond doubt that many natives of that country eat arsenic, with very remarkable benefit to their power of ascending steep mountains. It enables them to do this comparatively without distress to breathing and circulation. There are, however, a considerable number of persons who cannot take arsenic in the doses and for the prolonged period which are necessary to make any permanent impression upon either of the three neuroses of which we have been speaking. These patients suffer such irritation of the stomach or bowels from it, that they cannot continue it, For the relief of such persons, I beg leave again to suggest a remedy that I mentioned in my paper on Visceral Neuralgias in 1868, and which I have since found very useful in spasmodic asthma, as well as in gastralgia and angina ; I mean the subcutaneous injection of strychnia in very small doses, from the 1-120th to the 1-80th or the 1-60th of a grain. I would always try the subcutaneous administration before giving the remedy by the stomach, wherever it is possible to do so ; but, if this cannot be done, strychnia should be given by the mouth in doses of 1-40th to 1-24th of a grain three times a day. I think there is little doubt that strychnia also has a physiological affinity for the tract of the vagus and the trigeminal nerves ; also this is much feebler than that of the arsenic ; hence I should always recommend that the latter be tried first.—*British Medical Journal, Nov.* 11, 1871, *p.* 550.

---

### 37.—A FEW REMARKS ON THE TREATMENT OF ASTHMA.

By Dr. JOHN C. THOROWGOOD, Assistant-Physician to the Hospital for Diseases of the Chest, Victoria Park.

Experiments made by Paul Bert, Traube, and others, have shown that one result of irritation of the pneumogastric nerve

is to cause contraction of the lungs and arrest of respiration; this arrest being more easily produced during expiration than during inspiration.

The lungs themselves contract just as does a sponge when squeezed in the hand, and so long as this contraction continues so long does the paroxysm of asthma and difficult breathing exist.

Prolonged expiratory efforts, as by a violent burst of coughing or a fit of laughter, are very apt to determine a fit of spasmodic asthma in one predisposed thereto, illustrating the fact shown by the experiments above alluded to, that arrest of respiration from closure of the lungs is more easily produced during expiration than during inspiration.

This will be enough of the pathology of asthma for my present purpose, since I have shown what a fit of asthma is in itself, and that it may be caused by irritation of the pneumogastric nerve.

Now, this pneumogastric nerve, as its name implies, supplies nervous filaments not only to the lungs but also to the upper part of the alimentary canal—viz., the pharynx, œsophagus, and stomach, and hence any irritation of these parts may show its effects in the form of contracted lungs and asthmatic paroxysms.

These are matters tolerably well known and understood, and yet in the practical treatment of such a complaint as spasmodic asthma we seem hardly to get that help from this physiological and anatomical knowledge to which we are fairly entitled.

Efficiently to treat spasmodic asthma in its various forms we must recollect that we are dealing with a pulmonary neurosis, and endeavour to allay any irritation of the pneumogastric nerve as much as lies in our power.

Commonly we find in a paroxysm of pure spasmodic asthma that medication of those filaments of the pneumogastric that go direct to the air tubes and lungs by means of certain fumes and vapours, as, for example, burning nitre paper, or the smoke of stramonium or tobacco is sufficient to relieve the spasm and cause relaxation of the air tubes, but these are at best but temporary expedients, and naturally patients and physicians seek after some means of cure likely to be more radical and permanent in its effects.

So far as the experience of the present writer goes this may be found often in treatment carefully directed to the stomach and liver in the first instance; and, secondly, in the use of certain tonics that have in some way or other a permanently curative power over neuroses of the lungs and heart.

In observing patients with various chronic maladies of the lungs, we often have brought before us the sympathy that

exists between the pulmonary and gastric organs. For example, E. J., a man among my out-patients at Victoria Park Hospital, has been almost from childhood liable to cough and difficult breathing, and he remarks that he has always noticed any overloading of his stomach with food to be invariably and at once followed by increase of frothy expectoration.

Again, Mr. B. has had asthma for twenty years, and has taken all the most approved remedies for this complaint, under most able advice, with very little real benefit. He has, however, no trace of structural disease about the heart or lungs, while symptoms point very decidedly to irregular action of the liver and stomach. The occasional use of the *Cigarettes de Joy* and a mild dosing with Carlsbad salt, brought this gentleman to a degree of freedom from asthma that he had not enjoyed for full twenty years.

Much stress is very properly laid in books on asthma on the patient's diet. He must not dine late, and certainly must never indulge in supper. Sound and sensible as this rule is, yet I believe the practice of eating nothing after midday is carrying things a little too far in most cases. A state of exhaustion may be induced which always increases any kind of nervous irritability, and further than this, the long-continued innutrition may later in life lead to actual structural change of a degenerative kind in the lung tissue.

I find in practice that a small sandwich of bread and meat, or sometimes a cup of bread and milk, is very grateful to a hungry asthmatic in the evening, and in no way injurious.

The medicines that have more or less credit in the cure of asthma are almost "legion," the reason being that some fit, as it were, one end of the pneumogastric, some another. Thus bismuth and hydrocyanic acid are of great value when the neurosis is of gastric origin. Carlsbad salt, nitric acid, and at times small doses of mercury, are all unmistakeably curative when the hepatic system requires relief.

Other remedies, such as ipecac., belladonna, nux vomica, will cover a wider range of symptoms dependent on pneumogastric disturbance, while if we have reason to suspect a gouty or rheumatic diathesis as the root of the trouble, alkalies with iodide of potassium, sulphur, and arsenic will be the remedies indicated. While, then, we certainly have no "specific" for the cure of asthma, we may, by considering the complaint in its wide range, as a neurosis of the pneumogastric, get some very satisfactory results from treatment.—*Medical Press and Circular, Jan.* 10, 1872, *p.* 27.

## 38.—TREATMENT OF WHOOPING COUGH BY MORPHIA.

By Dr. John Kent Spender, Surgeon to the Mineral Water Hospital, Bath.

[The following two cases illustrate the advantage which may be obtained by the administration of morphia in extremely small doses, so as to get the sedative without the narcotic effect.]

May, 1863.—A young gentleman, æt. 10, suffering from the troublesome sequel of whooping cough; cough incessant, but the peculiar spasm of the specific disease had passed away. One twenty-fourth of a grain of acetate of morphia in solution was prescribed every two hours for the first two days; afterwards at longer intervals of time. The cough was notably alleviated almost immediately, and the patient enjoyed refreshing sleep during the first night. Scarcely any drowsiness was produced by the morphia, nor was any other inconvenience felt; not even constipation. Within five days the cough had nearly gone.

May, 1869.—A female child, æt. 5; parents not very poor; house clean and healthy. Whooping cough, rather severe; no complications. One thirty-second part of a grain of morphia with three grains of bromide of potassium, in solution, every two hours; mother was instructed to suspend the medicine for four hours at any time if unusual drowsiness came on. No medicine was given during the night. The usual auxiliary treatment as regards diet and clothing. The paroxysms of whooping cough soon became less severe, and the duration of the disease seemed shortened.—*Medico Chirurgical Review, Jan.* 1872, *p.* 217.

---

## 39.—ON THE THERAPEUTICAL VALUE OF THE HYPODERMIC INJECTION OF ERGOTIN IN HÆMOPTYSIS.

By Dr. C. Currie Ritchie, Physician to the Hulme Dispensary, Manchester.

In a suggestive paper published in the Practitioner, for Sept. 1868, Dr. Alfred Meadows advocates the extended use of this drug, which he had found extremely useful in various uterine and vesical affections; and in the same spirit I venture to submit the following observations on a therapeutical application of ergot, hitherto but little known, with the hope that the experience of others may be elicited, and so an accurate knowledge of its real value be arrived at.

It seems to me that the restricted use of ergot is the direct result of the want of a full appreciation of its great physiological action; that is, of the power which it possesses of inducing contraction in muscular fibre of the involuntary or non-striped

variety generally, and not merely in that of the uterus. Although having an important bearing on the general action of ergot, the following remarks apply especially to its action on the blood-vessels, of which unstriped muscular fibre is so important a histological element.

Experimental research has conclusively shown that ergot contracts the minute arteries, and by so doing increases the blood pressure. A simple experiment will, as Dr. Meadows has shown, be sufficient to establish the former of these propositions: thus, if a single grain of ergotin in solution be injected subcutaneously into the web of a frog's foot previously extended under a microscope, in a few minutes the circulation will be seen to become much quicker, then it will stop for a few seconds and oscillate in a spasmodic, jerking manner; after about half an hour, the blood-current will gradually return to its normal even, steady flow. The well-known experiments of Brown-Séquard afford additional evidence on this point. He found that in the dog the vessels of the pia mater contracted during its administration, just as with belladonna, but that ergot acted more powerfully on the cord than belladonna: both of these agents diminish the reflex power of the cord. The second proposition, that the blood pressure is increased, receives interresting corroboration from the recent researches, chiefly with the hæmadynamometer, of Dr. Ch. L. Holmes, published in the *Archives de Physiologie*, for 1870. He finds that "ergot contracts the minute vessels by an action on their muscular walls," and "that this contraction augments the blood-pressure in the large vessels;" it is, moreover, found that previous division of the vaso-motor nerves does not prevent the narrowing of the minute vessels, and that this diminution of calibre is also produced in the minute vessels of the lungs, causing a temporary diminution in the pressure of the systemic arteries. This last result has a direct bearing on the whole subject of pulmonary hemorrhages; and as it seemed to Dr. Holmes to be incapable of a scientific explanation, he performed numerous experiments in order to test its accuracy. Thus, he surmised that the effect might be due simply to the sudden introduction into the interior of the heart of a foreign body, and accordingly he substituted other substances for the ergot which he had injected into the jugular vein of a dog; but whereas the injection of ergot was with great rapidity followed by sudden diminution of the blood-pressure, which remained depressed for several seconds and then reached a higher level than before the injection, no such result followed the injection of the other substances. This sudden primary descent of the pressure-curve occurred after ergot-injection, even when it was introduced at a distance from the heart, and when the cardiac nerves had been

divided previous to the injection; so that Dr. Holmes is led to the conclusion that the occurrence is due to contraction of the pulmonary vessels, "impeding or even preventing the passage of blood into the left heart, and so diminishing the flow into the arteries and lowering their tension."

Such considerations as these lead us to hope that renewed and extended investigation by those who have the opportunity of inquiring into the therapeutical actions of the ergot of rye may assign this powerful agent its true place in our Materia Medica.

Ergot has long had the reputation of being a styptic; and as such has, in common with a multitude of others, been recommended in the treatment of hæmoptysis. Dr. Dobell (*Retrospect*, vol. lviii., p. 119) quotes a case in Guy's Hospital under the care of the late Dr. Addison, which proved very intractable until ergot was administered. Dr. Dobell himself recommends it, but administers at the same time ergot, digitalis, gallic acid, and other preparations, which leaves us in doubt as to which of the drugs was the means of arresting the hemorrhage. In a paper by Dr. Herman Weber (Clin. Soc. Trans. ii, 155), he remarks, "Another substance from which I have seen good effect in arresting severe hæmoptysis is ergot of rye in large doses, viz. from three to six drachms of the watery extract of our Pharmacopœia in twenty-four hours." I have not found the internal administration of ergot followed by the satisfactory results so frequently obtained from the use of gallic or sulphuric acids, acetate of lead, or sedatives; and in several cases it has produced such a disagreeable sensation of nausea that its use has had to be abandoned. In one case there was præcordial pain for half an hour after each dose.

An Interresting paper published by Von Langenbeck in the *Berliner klinische Wochenschrift*, 1869, "On the Hypodermic Injection of Ergot in Aneurism," contains the first account I can find of its administration by this method. He argued that, as ergot induced contraction of the smooth muscular fibres, it would be a powerful agent in producing hæmostasis. Two cases of aneurism, one in the right supraclavicular fossa and the other of the right radial artery, were successfully treated by this means. Dr. Dutoit of Bern (Langenbeck's *Arch.* Bd. xii. No. 3) and others have successfully repeated this method of dealing with aneurisms. In some editorial remarks appended to the medical periscope of the Edinburgh Medical Journal for July 1870, a case is quoted where a patient who had bled to thirty-six ounces from the lungs, within a few minutes, had his hemorrhage completely checked in three minutes by the subcutaneous injection of five grains of ergotin; and Dr. Allan Jamieson records a case (*Retrospect*, vol. lxiv, p. 108) in which

the same treatment was successfully adopted in a slighter case. The last case attracted my attention to this mode of treatment, and I have since adopted it in eight cases. The ergotine used was obtained from Messrs. T. and H. Smith, of Edinburgh, and a solution of five grains in ten minims of distilled water was employed, except in Case 8, in which unusual irritation was produced by it; I therefore substitute a solution (prepared according to Langenbeck's formula) of three grains of ergotin, in equal parts of glycerine and rectified spirit, which produced very little irritation.

[These eight cases are then related. In the first, five grains of ergotine were injected, with the effect of at once arresting severe hæmoptysis which had continued nine hours. The third was a case of phthisis, in which "pints" of blood had been expectorated, according to the patient's statement. There was a dry cavity at the apex of one lung, and the hemorrhage had been excited by the exertion of pulling on the boots. It had continued two days. The same dose of ergotine was injected as in the first case, with the effect of arresting the hemorrhage for two days, when a repetition of the injection permanently stopped it.]—*Practitioner, Dec.* 1871, *p.* 321.

---

## DISEASES OF THE ORGANS OF DIGESTION.

---

### 40.—ON DYSPEPSIA OF LIQUIDS.

By Dr. John C. Thorowgood, Assistant Physician to Victoria Park Hospital for Diseases of the Chest, and Lecturer on Materia Medica at Middlesex Hospital.

The cases here briefly recorded have seemed to me good illustrations of that indigestion of liquids which has been carefully described by M. Chomel, in his work on Dyspepsia, published in 1857.

The affection, in its fully developed form, does not seem very common in this country, but during the last few years I have met with a few cases exactly corresponding to the description of the malady given by Chomel.

*Case* 1.—James McC., aged thirty, a pale, dark, intelligent man, came under my care September 14th, 1861, complaining generally of dyspeptic symptoms, and specially of the great uneasiness caused by the presence of any amount of liquid in his stomach. Liquids in the slightest degree acid were most distressing to him, and at intervals he had attacks of sour pyrosis. He complained much of dryness of mouth, with dry skin and costive bowels; urine loaded with lithates, but free from traces alike of sugar or albumen. No loss of flesh; pulse

slow and soft; nothing irregular to be found in heart or lungs; no sort of tumour or thickening about pylorus of stomach, but on gently vibrating this organ, fluid was heard splashing about within it, and this sound could be always produced irrespective of any liquid recently drunk. The stomach was much distended. The early treatment of this case consisted in the use of alkalies, with bismuth and various bitters, but no improvement resulted; the only noteworthy feature was the effect of a pill of extract of opium at night, which regulated the action of the bowels so completely that the patient asked for the pills as *aperient* pills.

Often at his visits this man drew my attention to the quantity of liquid always present in his stomach, and at last we agreed that he should drink as little as possible, and take no other medicine than a powder of rhubarb and magnesia every morning. From this time he steadily improved, and after about two months' treatment he was let go cured, and six months afterwards was still in good health.

*Case* 2.—A man aged fifty-three years, dark, sallow, and very hypochondriacal, had been dyspeptic for eighteen months, and had taken quantities of physic. He often has pyrosis, has large sodden tongue, costive bowels, and dry skin; urine dark in colour, specific gravity 1030, acid, and nothing abnormal chemically or microscopically. The indigestion of liquids was not so marked in this case as in the one preceding, but no medicine gave relief till the dry plan of diet was enforced, then he amended considerably, and I lost sight of him.

*Case* 3.—A young healthy-looking man, aged twenty years, by trade a bricklayer, came to me complaining of faintness and palpitation of the heart, and said he had got water in his chest, for he could feel it. Examination of the chest showed the lungs and heart to be in every respect quite healthy; the stomach is large and dilated, and when he shakes the splashing noise of liquid therein is plain enough. He says he can produce this noise at any time. This patient has a dry skin; large sodden tongue; pulse 80; urine clear. No water-brash.

So far as treatment has gone during one month with alkalies, bismuth, and various tonics, little good has been done. The dry-diet plan is the only means of effecting a regular cure; but as this involves a pretty complete abstinence from beer, the patient can hardly be made to give it a fair trial. That the less he drinks the better he is however, is a fact already well proved.

At first sight this sort of case may appear of a pretty ordinary kind; but what I claim as their characteristic features are these:—They are cases of obstinate dyspepsia, marked specially by the discomfort of feeling the stomach always dis-

tended as with fluid, while the mouth is usually dry, thirst troublesome, and yet drinking always aggravates the discomfort. There is no acute pain, and the patient rarely gets thin. Movement of the patient produces that splashing noise or "*clapotement*," as Chomel calls it, significant of the presence of wind and water in the stomach; and this can be produced at any time by the patient. Lastly, there is the almost certain amelioration of the symptoms under the influence of the dry plan of diet; and till that is followed no medicine does any good whatever.

It is necessary to seek carefully for any organic disease, for if there be stricture or thickening about the pylorus it will not add much to the doctor's credit to promise a certain cure if the dry plan of diet is followed; relief there may be, but not cure. Chomel says of one of his cases; "J'ai vu un autre individu, atteint de dyspepsie des liquides, qui, en appuyant avec quelque force la main sur l'epigastre, faisait remonter dans sa bouche plusieurs onces d'un liquide aqueux; mais ce fait a été unique pour moi." I have sought to elicit this symptom in the cases recorded, and in others, but have not been able to produce it.

At times fits of fainting, with irregular action of the heart, are prominent symptoms in these cases of dyspepsia of liquids, the cause being due to the distended state of the stomach. Marked relief is obtained to these symptoms on the dry-diet plan, and this confirms the soundness of the practice of not allowing patients with heart disease to live much on liquids and slops, but rather to feed them on easily digested solids, such as mutton, chicken, boiled fish, and game. These are all forms of solid food, suitable in cases of dyspepsia of liquids. Milk does not seem otherwise than beneficial, but much of farinaceous or vegetable food should be avoided.

The patient must bear a certain amount of thirst as well as he can, and when he does drink must take but very small quantities at a time, and not drink for an hour or more after he has taken his meal of solid food. Weak whisky-and-water, sherry wine, and toast-and-water, are among the least objectionable drinks; and sometimes a small cup of pure beef-tea, free from any farinaceous admixture will suit well. One of my patients found a wineglass of good stout to agree well, and relieve his thirst.

Compared with diet, medicine is but a secondary matter; but a dose of Gregory's powder in the morning and a tonic mixture of rhubarb and tincture of nux vomica twice a day, I have found serviceable. Mild saline purgation I have tried, but its effects are very uncertain, relieving at times slightly, at other times adding decidedly to the discomfort, and it is not a method of treatment to be recommenced. A pill of opium at night, when

there is much nervousness and hypochondriasis, is certainly beneficial, and a pill of bitter extract or of galbanum and assafœtida twice a day may be used with advantage.

It is rare for the patients to emaciate, provided the case be uncomplicated. In one of Chomel's cases, however, he noticed a degree of emaciation that caused great anxiety, being an unusual symptom, and likely to indicate organic visceral disease. The dry regimen was enforced, and the lady got quite well, though any over-indulgence, even in such a fluid as plain water, would be followed by return, in some degree, of the unpleasant symptoms.—*Lancet, Feb.* 17, 1872, *p.* 215.

---

## DISEASES OF THE URINARY ORGANS.

---

### 41.—TABLE FOR THE EXAMINATION OF URINE.

By Dr. J. Campbell Brown, Lecturer on Chemistry and Toxicology at the Liverpool Royal Infirmary School of Medicine.

I.—Observe the colour and appearance of the urine, whether it is clear or turbid, and whether it contains much mucus.

A high colour may be due to Bile, Blood or Purpurine; a pale colour may indicate excess of Water, and frequently also Glucose.

II.—Observe the reaction to red and blue litmus papers.

Normal urine is slightly acid; if the reaction is alkaline, and the red colour of the paper is restored on drying it, the alkalinity is probably due to ammonium carbonate from the decomposition of urea; confirm by observing whether effervescence occurs on the addition of an acid to the urine.

III.—Observe the specific gravity.

*a.* If the specific gravity is above 1025, test for glucose by (1.) potash solution and heat; glucose gives a dark solution. (2.) Add potash and filter, if necessary, then add copper tartrate and more potash until a blue solution is obtained; on heating to the boiling point glucose reduces a red or orange precipitate of $Cu_2O$.

*b.* If the specific gravity is high and sugar is not present, add to a portion of the clear urine in a deep watch-glass about one-half its volume of cold concentrated nitric acid; a deposit of hexagonal plates of urea nitrate indicates excess of urea. (Probably excess of phosphates and other salts will be found accompanying excess of urea).

*c.* If the specific gravity is below 1012, this may be due to great dilution of the secretion with WATER, which will be further indicated by the large quantity passed in twenty-four hours; but it is more generally due to disease of the secreting organs, and is accompanied by albumen, the urine being then frequently alkaline, but sometimes acid.

IV.—Heat a portion to the boiling point in a test tube, albumen may be at once coagulated; add nitric acid drop by drop; a flocculent precipitate indicates ALBUMEN; confirm by adding to another portion of the urine acetic acid, filtering to remove mucus, if necessary, and then adding potassium ferrocyanide; a white precipitate indicates ALBUMEN. The deposit from an albuminous urine should be examined microscopically for CASTS, PUS and BLOOD GLOBULES.

Boiling alone may first cause a precipitate of CALCIUM PHOSPHATE, which will be re-dissolved on the addition of nitric acid. If a turbid urine is rendered clear by boiling, the turbidity is due to urates.

V.—Add to a portion of the urine, ammonia in excess; the white precipitate consists of ALKALINE-EARTHY PHOSPHATES; filter and add ammonium chloride and magnesium sulphate; the white crystaline precipitate indicates the amount of phosphate which was originally present as ALKALINE PHOSPHATES.

VI.—To another portion add ammonia and filter; then add ammonium oxalate; the white precipitate contains the CALCIUM as oxalate.

VII.—To another portion add nitric acid; divide into two parts; to the first add barium chloride; the precipitate contains SULPHURIC ACID as barium sulphate. To the second add silver nitrate; the curdy precipitate contains the CHLORINE as silver chloride.

VIII.—A dark brown or blue colour may be due to INDICAN which is destroyed by nitric acid.

Any colour from that of Gregory's powder to an olive green tint may be due in part to bile.

(1.) Pour a layer of the urine (concentrated, if necessary,) on to a white dish, and add concentrated nitric acid. A play of colours, green, blue, purple, and red, indicates BILE PIGMENT.

(2.) Boil a portion of the urine with acetic acid, and filter to remove albumen, then add a few crystals of cane sugar, and a few drops of concentrated sulphuric acid; a purple tint indicates the ACIDS OF BILE.

A red colour may be due to blood; in this case heat will have destroyed the colour, and coagulated the albumen of BLOOD. Examine
(1) by the microscope for BLOOD GLOBULES, and
(2) by the spectroscope for HÆMATINE.

A high colour may also be due to purpurine. In this case it is unaltered by heat and by nitric acid. Boil a portion with hydrochloric acid. A dark red or purple colour indicates excess of PURPURINE, of which a small quantity is present in normal urine. Allow to stand for a day; the crystals which slowly form are URIC ACID, an excess of which frequently accompanies purpurine.—*Liverpool Medical and Surgical Reports, Oct.* 1871, *p.* 42.

---

## 42.—ON THE TREATMENT OF DIABETES BY LACTIC ACID. (CANTANI'S METHOD).

By Dr. GEORGE WILLIAM BALFOUR, Physician to the Royal Infirmary, Edinburgh.

[Dr. Balfour at the commencement of his paper gives a *resumé* of the theories of diabetes, and the modes of treatment founded thereon.]

Now, without entering more at large into the pathology of diabetes, it is obvious that these various modes of treating it are all underlain by one common fact—viz., that in diabetes the natural glycogenic function is discharged in a morbid manner, and that this morbid discharge of function may be importantly modified in two modes; 1st, by depriving the organism of that pabulum from which the glucose is mainly derived, and 2nd, by employing such remedies as are capable of altering the nervous energy of the organs at fault: practically it has been hitherto found of importance to conjoin these two methods.

The chemical treatment of diabetes by the attempted burning the sugar by artificially induced hyperoxygenation of the blood, has not been found more successful than the simple dietetic plan without this addition, and for the chemical reason stated this could not be otherwise. While the basis of the other chemical treatment, that by alkalies, has been shown to be erroneous, the blood of diabetic patients being not less alkaline than that of others on the one hand, while on the other, a fluid even more alkaline than blood cannot decompose glucose, so that if the alkaline treatment have really proved more successful than the simple dietetic plan, that must have depended on its exercising some modifying power over the function at fault—a modifying power, however, of so doubtful a character that it has not been able to prevent this method of

treatment from falling, in this country at least, into comparative desuetude.

It is a matter of daily observation, that morbid alterations of function are frequently only to be permanently remedied by a restoration of the body to its normal standard, the great difficulty of promoting a cure in such cases being the impossibility of giving perfect rest to one organ while endeavouring to build up the frame, the skill of the physician being shown by the manner in which he solves this complicated problem, giving as much rest as possible to the organ at fault, while at the same time improving the general nutrition, and thus restoring the healthy tone to the constitution. It is in this respect that all the treatments hitherto propounded for diabetes have failed; they may have given rest to the organ at fault, but they have too often depressed the general health, and, while giving temporary relief, have possibly in many cases hastened the end; and even when a tonic treatment has been mainly relied on it has failed for want of physiological adaptation. It is in this respect that Cantani's treatment is pre-eminently useful; it relieves without depressing, it gives as nearly as possible perfect rest to the organ at fault, and at the same time builds up the body by an artificial adaptation of physiological means, and restores the healthy tone to the constitution, thus enabling it to return to healthy action. It is by no means put forth as a perfect cure for all cases of diabetes; it is a means of relief to all, but only a cure for some; yet it seems to act curatively in a larger proportion of cases than any other mode of treatment hitherto devised, while the relief to all is more perfect.

Professor Alnaldo Cantani, of Naples, agrees so far with Reynoso in believing that in diabetes the question is not so much one of increased production of sugar as of defective combustion; this he conceives to be proved by the small number of respirations made by diabetic patients and by by their average low temperature, ranging, according to him, from 95° to 96°8 F. in uncomplicated cases; the temperature, of course, rising with any complication—such as tuberculosis—giving rise to pyrexia, but always remaining below the normal of such pyrexia. This defective cumbustion he believes to depend upon the production of a morbid form of glucose, which he has termed *paraglucose;* this is incapable of being transformed into lactic acid, and therefore cannot be burned; it is consequently passed unchanged in the urine. The result of this is, that the sugar and starch of the food being transformed into this incombustible paraglucose, the heat of the body is in so far maintained at the expense of the albuminates and fats, and from the combustion of the former we have that excess of urea which adds so greatly to the density in many cases of diabetes, while, as

the albuminates and fats which the patient receives as food are insufficient for his requirements, those of his own tissues are also employed, and hence his rapid emaciation.

In the early stages of diabetes the quantity of sugar passed in the urine oscillates with the diet, and with an exclusively animal diet is much lessened in twenty-four hours, and may entirely disappear in a few days; in the later stages, when sugar persists in the urine even after the withdrawal of all amylaceous food, Cantani believes that not only the inosite of the muscles may be transformed into paraglucose, but also that the gelatinous tissues, which Baedeker succeeded in transforming into sugar apart from the system, may in this anormal pathological condition become transformed into paraglucose, and thus account for the persistence of sugar in the urine of those fed exclusively on an animal diet apart altogether from the question of the albuminates themselves being transformed into diabetic sugar. Thus, though all diabetics are to a certain extent autophagic, we may yet have this abnormality developed to excess, and may thus have three steps in this disease: first, that in which the sugar oscillates with the quantity of amylaceous food supplied; second, that in which the inosite and gelatinous matter of the animal food are transformed into diabetic sugar; and, third, when not only these but also the inosite and gelatinous matters of the body of the patient himself are so transformed—a most severe and hopeless form of autophagia. Cantani, with most modern pathologists, recognises the liver as the organ mainly at fault in these cases; but whatever the organ may be, he proposes to give it as complete a rest as possible, by depriving it of its pabulum in subjecting the patient to a rigorous meat diet, thus reducing to a minimum the introduction into the system of the sugar-producing substances. But as that is only a temporary expedient, having reference to only one element of this disease, and incapable of arresting the waste and ultimate complete degradation of the body, he further proposes to prevent this waste by supplying a cumbustible agent in a quantity sufficient for the wants of the body, so that the fats and albuminates may continue to be stored, and the body thus gradually restored to its normal standard; and he hopes that this restoration of the healthy standard of the constitution, coupled with the prolonged functional rest to the organ affected, may suffice to prevent any relapse into its morbid condition even after a return to the ordinary dietetic conditions of modern civilized life. The result of Cantani's experiments have to a large extent proved the correctness of his views. The combustible agent which Cantani has selected is lactic acid, and it has been wisely chosen, inasmuch as it is in all probability that agent employed in the normal

conditions of nutrition, representing as it does the intermediate stage between glucose and carbonic acid, so that not only is a complete rest given to the organ at fault, but that very agent is supplied to the system which would have been normally present had the organ at fault been doing its duty after a normal fashion, so that while as complete a relief as possible is afforded to the organ at fault, there is in no respect any anormal strain put upon the system. The organ at fault is for the time being merely thrown out of gear, but all the other functions proceed in a natural manner, so that, when the normal tone of the organ has been re-acquired, it may again be replaced in the natural cycle without the harmony of the natural sequence of the functions being in any way interrupted either by the cessation or the restoration of its function. The quantity of lactic acid which Cantani administers is from 77 to 154 grains daily, diluted in from 8 to 10 fluid ounces of water, and taken during the day. His exclusively meat diet means rigorously one of plain meat, roast or boiled, without any sauces of milk or eggs, and certainly without any bread, flour, or any vegetable matter whatever, the only seasoning permitted being salt, oil, and a little vinegar. For drink he allows water, either plain or with a little of the purest alcohol; coffee, tea, and wine being prohibited. His results have been somewhat surprising. In recent cases the cure is stated to be almost certain and speedy; and even where an exclusively meat diet is not persisted in, life is apparently prolonged, and many of the unfavourable results of diabetes are prevented, though the mellituria is not arrested. Dr. Sammut of Naples, from whose report I quote, states that he has seen an army lieutenant enter the hospital emaciated, weak, and impotent, with polyuria, thirst, and extraordinary hunger. In twelve days the last trace of sugar had disappeared from the urine, and in six weeks the patient had gained nine pounds. He left the hospital in excellent condition—florid, strong, without thirst or hunger; he continued the treatment for two months more at home, and then resumed mixed diet, and after the lapse of eight months he was in excellent health, and weighed twenty-one pounds more than when he entered hospital, being also without a trace of sugar in the urine, though eating indiscriminately of all that came before him. A tailor from Naples was admitted in a desperate condition. He left florid and robust. Since then, for more than seven months, he has been eating promiscuously, and is more fat, rubicund, vigorous, and energetic than before his attack. Dr. Sammut also states that he saw in Cantani's clinic several other diabetics—he mentions five—all much improved, and several believing themselves to be perfectly cured.

These results are certainly very remarkable, and being a con-

tinuous series following the employment of a treatment based on rational considerations, are sufficient to claim the attention of every medical man interested in the progress of therapeutics. And I may remark that the latest treatment of diabetes propounded in this country—that by skim-milk—bears out Cantani's views in so far that it is a strictly animal diet, free from amylaceous matter, and containing three to six per cent. of lactin, which, under the influence of the caseous matter, becomes transformed into lactic acid. This treatment is therefore an approach, but a very meagre one, to Cantani's apparently more perfect system; and I may add that the results of the two systems in my own practice have fully convinced me of the greater applicability and more perfect success, so far as time allows me to judge, of Cantani's method.

The first case in which I employed it was not a favourable one. The patient had been formerly much benefited by the conjunction of opium in large doses with the dietetic treatment. He returned two years afterwards with commencing phthisis, and though benefited somewhat by the lactic acid treatment, yet he died, as was naturally to be expected. His urine fell in five days from an average of about 250 ounces to 70 ounces in the day, the specific gravity remaining about the same—1·030; this high specific gravity being apparently due to the amount of urea and uric acid present, a condition of matters usual in this treatment. The deposit of red oxide of copper following the application of Trommer's, which had been copious, ceased entirely, and was replaced by a brownish discoloration due to the excess of uric acid present. No change took place on boiling with liquor potassæ (Moore's test). The poor man therefore lost the main symptom of his disease, but he died exhausted after about three months' treatment.

*Case* 2.—David Hume, an engineer, aged 53, was admitted into Ward V. on July 14, 1871, complaining of great thirst, debility, and polyuria. Three years ago he suffered from a stroke of paralysis which affected the right arm and leg. He was able to resume work three months subsequently, but still complains of weakness in the right leg and dulness of hearing in the right ear. His integuments are rather dry, his joints normal, but his limbs are much emaciated, and he sleeps badly at night. Sensation and intelligence normal, but motion much impaired from his great exhaustion, having had to rest frequently in coming upstairs to the ward. He measures 5 ft. 7 in., and weighs only 8 st. 8 lbs. His tongue is dry in the centre, moist and clammy at the tip and edges, red, and glazed. His mouth and fauces dry. He has a constant bad taste in his mouth. His teeth are decayed. His digestion good, but his bowels usually constipated. His arteries are atheromatous, but

the circulatory and respiratory systems otherwise normal. He calculates his total drink at 100 ounces daily, but he passes between 300 and 400 ounces of a pale-coloured urine daily, spec. grav. 1·030, and showing a large amount of sugar by the ordinary tests. He states that he has been under medical treatment for upwards of a year, but has nevertheless been gradually getting worse, and he dates the commencement of this illness between three and four years ago. Up to the 15th of August he received only ordinary diet and no special treatment, but this day he was placed upon a strict meat diet, consisting of 2 lbs. of boiled meat and 2 lbs. of steak in the twenty-four hours, which was the quantity he found to be sufficient. He was also ordered one teaspoonful of lactic acid in a tumblerful of water, to be taken in quantities sufficient to assuage his thirst. On the 29th of August his urine still contained sugar, was reduced, however, to 86 oz. in the twenty-four hours, his total drink being now stated to be 78 oz. This day he was ordered a bottle of diabetic koumiss in the day. Of this he, however, only drank about half a bottle daily. On Sept. 1—his total drink being 48 oz.—he passed 48 oz. of urine rather deeper in colour, of spec. grav. 1·025, and containing no sugar. On Oct. 1, the patient, having continued to improve uninterruptedly, was permitted to add to his diet four so-called bran biscuits, which contain, however, a large amount of starch, and a cabbage. He takes his acid regularly. His total drink is now 36 oz., his total urine passed 40 oz., spec. grav. 1·025, and no trace of sugar. On Oct. 20, two additional bran biscuits were granted to him; and on Oct. 25 he was permitted to breakfast on porridge and skim-milk, the bran biscuits being stopped, and his meat reduced to 2 lbs. daily, which is found sufficient. His weight is now 10 st. 7 lbs., and his urine keeps free from sugar. He was now put under surgical treatment for a stricture of the urethra, the lactic acid being continued, and bran biscuits gradually added to his porridge and skim-milk and meat diet, till he took six biscuits in the day. His drink and urine both averaged about 40 oz. in the twenty-four hours, the urine keeping free from sugar. On Nov. 14, his weight was 11 st. in his drawers alone, and, feeling quite well, he was discharged. He was shown at the Medico-Chirurgical Society on the evening of Nov. 15, and has orders to report himself occasionally, so that his further progress may be ascertained.

*Case* 3.—Mary A. Early, a mill-worker, aged 15, admitted into Ward XIII., July 18, 1871, complaining of weakness, general malaise, polyuria, and diarrhœa, states that her present illness dates only from four weeks ago, when she was seized with headache, which lasted all day, nausea, great thirst, and

diarrhœa. Her appetite being unimpaired, her previous health was good, and her family history unimportant. On admission her skin was hot and dry, limbs much emaciated, height 4 ft. $8\frac{3}{4}$ in., weight 4 st. 11 lbs. Complains of headache and great thirst. Her tongue is covered with a slight brown fur, moderately moist. Her bowels loose, several stools being passed daily. Her urine is pale, spec. grav. 1·030, and contains a large amount of sugar; its quantity is much increased, but cannot be measured on account of the diarrhœa present. Other symptoms normal. The diarrhœa was treated with chalk and opium mixture, and ceased in a few days. Her drink was now found to vary from 100 to 170 oz. daily, and the quantity of urine passed from 120 to 180 oz. On July 27, she weighed 4 st. 7 lbs., and was placed upon an *ad libitum* allowance of meat, of which she managed to consume about 2 lbs daily, no extras. She had a bottle of soda-water daily, and two teaspoonfuls of lactic acid in a tumblerful of water as often as desired. The very next day her total drink sank to 32 oz., and her total urine passed to 22 oz. daily; and on July 29 all trace of sugar ceased to be discoverable, spec. grav. of urine remaining at 1 030, but containing a large quantity of uric acid and crystals of oxalate of lime; total quantity passed 20 oz., total drink 40 oz., in the twenty-four hours. After this she continued to drink with almost unvarying regularity 40 oz, in the day, passing from 28 to 40 oz. of urine, averaging 33·5 daily for the three months, August, September, and October. On Aug. 7, she weighed 4 st. 6 lb., having lost 1 lb. since being put upon her restricted diet, and on the 12th her weight remained the same. She says she feels quite well, but is unable to walk much or even stand long from a feeling of weakness across her loins. Aug. 19. Patient weighs now 4 st. 7 lbs., spec. grav. of urine 1·030, no sugar. On Aug. 12, she was offered cabbage or green vegetables, which she declined, and was then allowed to get half a slice of toast in the day; but this was stopped on the 20th, on account of a slight trace of sugar which was found in the urine on that day. On Aug. 25, the patient weighed 4 st. $8\frac{1}{2}$ lbs., and was permitted to have some diabetic koumiss for drink. She did not like it, however, and only managed to drink three bottles of it between this date and Oct. 3, when it was stopped. On Sept. 17, patient weighed 5 st., and sugar having remained absent from the urine from Aug. 12, she was allowed to have half a slice of brown bread daily. On Sept. 30, patient weighed 5 st. 5 lb., a marked quantity of sugar was found in her urine, but as it was distinctly traced to her having eaten a slice of white bread and jelly which she had got from another of the patients, she was permitted to continue the brown bread. Oct. 9. Weighed 5 st. 7 lbs. Brown bread stop-

ped on account of slight discoloration of the urine when boiled with liquor potassæ. Oct. 14. Patient weighs 5 st. 9 lbs., and on 14th Nov. 5 st. 10 lbs., her height being 4 ft. $8\frac{3}{4}$ in. Oct. 17. The urine keeping free of sugar, the patient was allowed to have four bran biscuits in the day, which contain a notable quantity of starch; and on the 27th these were stopped, and porridge and skim-milk ordered for breakfast. About the end of a week, however, a trace of sugar reappearing in the urine, the porridge was stopped, and for another week she was relegated to her meat diet, of which she takes about two pounds daily. After doing penance for one week she was again permitted to have her four bran biscuits, which she has continued to this date. Nov. 16. Her urine keeping free from sugar, 36 oz. in quantity, and spec. grav. 1·030. Obviously this girl is far from being in the same satisfactory condition as Hume, but, considering her youth, and the well-known obstinacy of diabetes in youthful patients, I think there is every reason to be thankful for the improvement she has made, and also strong grounds for hope that a more prolonged treatment will ultimately result in her restoration to health.

The lactic acid I have employed has been that obtained from the druggists, fluid, not syrupy, of a spec. grav. of 1·027, and with the ordinary musty smell of sour milk. Three to four drachms in the day seem to be quite sufficient for all practical purposes; yet much more may be taken without detriment, cases 2 and 3 occasionally taking as much as eight or nine fluid drachms in the day. The remedy is, however, as yet rather too expensive to be employed in larger doses than are absolutely necessary, as it costs ten shillings a pound; a more extended application will, of course, speedily cheapen its production.

The great advantage of this treatment is the prospect which it holds out of a comparatively speedy restoration to ordinary mixed diet, with persisting absence of sugar from the urine—of rapidity of cure, in short; and this rapidity is in itself a great encouragement to such patients steadily to persevere, in spite of the irksomeness of a diet rigidly restricted to meat, which seems certainly to be required for two or three weeks at least, after which, fish, fowl, and green vegetables may be added, and other relaxations gradually and cautiously permitted, provided the case progresses favourably. I fancied that the koumiss was of considerable benefit in Hume's case, and if its composition be correctly stated, it ought to be a useful diet for diabetics, but a rigid analysis is still requisite. It is prepared by Chapman of Duke Street, London, and is said to consist of milk, fermented in a warm place by the addition of a small quantity of very sour milk; it effervesces slightly, and is pleasant to the taste, being apparently composed of the caseous matter in a state of

extremely fine division, lactic acid, and a certain amount of alcohol, besides the fatty matter and salts ordinarily present in milk. The only apparent drawback to it is the statement that in cold weather it may act as a diuretic—an action which is certainly not required in diabetes.

Bonthron's diabetic biscuits are perfectly palatable, and I fancied they might prove a welcome addition to the very limited dietary permissible under Cantani's treatment. They are said to be prepared from finely-ground bran, washed as free from starch as possible, and mixed with albumen and butter. I procured some of these biscuits, and had them analyzed by Dr. Stevenson Macadam, of Edinburgh ; and the analysis shows them to be wholly inadmissible in the early stages of this treatment, while, after a certain stage has been reached, they are not required, the ordinary bran biscuits of the shops, which are much more palatable, being perfectly sufficient, and forming an admirable introduction to more mixed dietary.—*Edinburgh Medical Journal, Dec.* 1871, *p.* 533.

# SURGERY.

## AMPUTATIONS, FRACTURES, DISLOCATIONS, AND DISEASES OF BONES, JOINTS, &c.

### 43.—ON THE USE OF THE BAVARIAN APPARATUS IN THE TREATMENT OF FRACTURES.

By Dr. ANTHONY H. CORLEY, Surgeon to Jervis Street Hospital, Dublin.

[This method of treating fractures was described at the time of the Franco-German war. (See *Retrospect*, vol. lxii, p. 172, also vol. lxiv, p. 145.]

The appliances are few and inexpensive. A yard of the cheapest flannel, a pound or so of plaster of Paris, a few large pins with their heads bent, and a piece of calico or common roller make the surgeon independent of the surgical-instrument maker. The flannel is cut into two rectangular pieces the length of the fractured bone, and broad enough to encircle the limb and to leave a margin. One is a little wider than the other. Placing the narrow one evenly over the other they are sewn together by longitudinal stitching down the mesial line. They now resemble two sheets of note paper stitched together at the fold, the outer a little larger than the inner.

Raising the fractured limb carefully the flannel is to be spread smoothly under it, taking care that the line of sewing corresponds to the posterior mesial line of the limb. The two ends of the inner sheet are now brought evenly over the limb and fastened together by means of the bent pins, leaving the two outer half sheets spread on the surface of the bed. By extension, counter-extension, and manipulation, exact coaptation is secured, and now the plaster, mixed to a proper consistence with water, is partly smeared and partly poured on. The two outer sheets of flannel are rapidly brought over the surface of the plaster (which is now caught on both sides between the inner and outer layers) and are held together at their margins till the plaster sets, taking care that the extension and counter-extension are kept steadily up during that period. The pins must now be taken out (and it is for this purpose that their heads are bent), the edges trimmed, a few turns of the roller,

or this kind of bandage (the use of which I have learned from my senior colleague, Dr. Stapleton) now being applied, and the entire operation, which does not occupy ten minutes, is finished. Muscular spasm at once ceases, the patient acquires the power of shifting his position, and a steady equable lateral pressure is secured, adapted exactly to the shape and inequalities of the limb.

Should it be necessary to maintain any peculiar position, as in the treatment of Pott's and Colles's fracture, the assistant holds the limb while the plaster is setting, and a firm rigid case is at once secured, exercising uniform pressure, incapable of slipping, and maintaining the fractured ends in steady apposition. But the most important advantage connected with this apparatus is the facility with which it may be taken off. Most plaster and starch bandages are not only troublesome to apply, but so difficult to remove, that there is a strong natural tendency on the part of the surgeon to leave them on, and trust that the bones are uniting properly. I believe I am not the only surgeon who has seen, when the bandages are cut, certain peculiar angles and curves in the limb that Nature never intended to be there. Again, there are certain fractures, as compound ones, &c., in treating which we must keep a portion of the limb in such a way as to be readily examined. In the Bavarian apparatus, when the bandages are loosened, the two opposite sides can be separated, like the bent covers of a book, the line of stitching (which prevents the running together of the plaster) acting like the back or hinge of the book, and allowing the limb to be exposed or to be taken out if necessary. If the swelling, usually present at first, diminish very much, the anterior edges can be trimmed so as to allow tightening, or the whole case may be taken off and lined with cotton. I have used it *immediately* after accidents, and the only cases where I should defer its application are those of fracture from direct violence where there is such contusion that even the most equable pressure would be contra-indicated. I have used it with success in fractured femurs, tibia, fibula, humerus, radius, ulna, as well as with Pott's and Colles's fractures, and have seen it used equally successfully by my colleagues, Drs. Stapleton and Walshe. When I say that Dr. Stapleton stamps it with his approval, I may at once claim attention for it. It is however with special reference to its efficiency in the treatment of Pott's fracture, Colles's fracture, and fracture of the shafts of the femur, that I now wish to speak. Everyone knows the difficulty of treating the first of these very common accidents. I have tried Dupuytren's splint applied with care by myself, and have had the pleasure in a day or two of finding the splint slipped round to the back of the limb, a circumstance highly

calculated to allow the fracture (to use the time-honoured witticism) to turn out remarkably well. I have used the same splint, placing the limb after its application in a box, and I have used the box alone, trusting to padding to keep the proper position. I have secured *tolerably* favourable results, but only by means of constant trouble, frequent re-adjustment, and exemplary patience. When the Bavarian apparatus is applied there is no further interference necessary, the patient can at once lie on his side, and get up to sit in a chair in two or three days.

With Colles's fracture I have since my attention was directed to Gordon's splint by Mr. Porter's paper, used none other until recently, and am perfectly satisfied with its superiority over every method of treatment before adopted. I have, however, tried how the plaster apparatus would suit, and have every reason to be pleased with the result. In applying it in this fracture I place a pad of cotton over the dorsal prominence, and hold the hand strongly adducted till the plaster is set. In the two cases of fractured femur which I have treated in the last six months I have secured such admirable results, and with such comfort to the patient, that I shall dwell at some length on the accessories to its successful use. I have always been under the impression that if in this fracture the ends of the bone could be got fairly into apposition and *kept together* by equable lateral pressure, there would not be the great necessity for extension and counter-extension now presumed to exist. The long splint, which enforces a constant recumbent attitude, and the use of a perineal band, has been felt to be such an imperfect mode of treatment, that dozens of modifications of it have been proposed, of which a few trials have convinced surgeons that the proper apparatus is still a desideratum. Counter-extension by the perineal band is particularly objectionable from its pressure, from the irritation or even the ulceration so likely to be produced, especially in children, and from its tendency in fractures high up to increase the deformity.

With the plaster apparatus the necessary lateral pressure is produced, and the only requisite afterwards is some means by which slight extension and counter-extension can be kept up, less I believe for the purpose of producing any effect at the fracture than for preventing the motions of the patient's body (which I freely allow) from disturbing the position of the fractured ends. Having adjusted the apparatus I apply two long strips of soap plaster to the opposite sides of the leg, uniting them in a loop below the foot; to this loop I fasten a cord, which passes over the foot-rail of the bed, and has a weight dependant. This is the extension. I then raise the

lower end of the mattress so that the feet are considerably higher than the pelvis, and the body forms the counter-extension. After the second day I found in both my cases that the patient could sit up to eat his dinner, &c., without pain increasing, or disturbing his limb in the slightest degree.

My second case is still under treatment, and through the courtesy of my colleague, Dr. Walshe, I am able to mention that there is at present in Jervis-Street Hospital a child, æt. 4, both of whose thighs and right humerus were fractured by being caught in the wheel of a car. All the broken bones were at once put up with the plaster apparatus. Immediately afterwards the little patient seemed quiet at ease, and free from suffering, and has since continued to progress. The efficiency of the plaster in this case, and the difficulty which the application of two long splints would present, speak volumes.

The cases which I have noted (some twenty in number) may seem too few to give me such confidence in the apparatus, but I trust, for the reasons I mentioned, the society will excuse my bringing this mode of treatment forward, and that my explanation and advocacy may secure for it a more complete and satisfactory trial.

[The following discussion took place when this paper was read before the Surgical Society of Ireland.]

The President (Mr. Wharton) said he had listened to the paper with great pleasure, and he was sure they all felt greatly obliged to Mr. Corley for bringing forward this mode of treating fractures. He had seen his colleague, Mr. White, use it. That morning he had had an opportunity of seeing such a case as Mr. Corley had exhibited the apparatus for. Nothing could be more expeditiously or efficiently done. It only took about five minutes to put up a fracture of the tibia. Mr. White had a stiff case for the patient's leg almost immediately, to the man's great comfort and relief. He had also seen Mr. White use a modified many-tailed bandage, which he called by Mr. Stapleton's name. All present who knew the great difficulty of getting a good bandage would appreciate it.

Mr. Stapleton said he had seen several cases treated with this apparatus by Mr. Corley, and he must certainly say that in Pott's fracture he considered it one of the greatest blessings to the patient, and one of the greatest improvements in surgery. He himself had for a long time discarded Dupuytren's splint. At the time it was brought before the society he combatted the great advantages that were claimed for it. He (Mr. Stapleton) used a box splint, with pads. If they put on Dupuytren's splint in the original way it would not do. It had been nicked and notched by surgeons to try to retain it in its

place, but notwithstanding it still slipped. But he had never seen such advantage as was given by this apparatus in Pott's dislocation. As to the many-tailed bandage he was first led to use it in fractures of the thigh. In fractures of that bone, put up in Liston's long splint, with the bandage round the foot and up the thigh, if you wanted to see what was going on you had to unroll the whole bandage. Any one who had had a number of fractured thighs under his care at one time would know how laborious this was. He had had eleven fractured thighs under his care at one time, and he then took this plan of using the old many-tailed bandage. If one part was loose it could be tightened, or all could be loosened to look at the fracture. In treating fracture of the thigh he used three splints. He never disturbed the leg, but simply undid the bandage, and saw what was going on. It might be said, "Oh, put on a starch bandage," but he had seen some very ugly accidents from leaving starch bandages on too long. Sometimes when a surgeon thought that he was going to have a nice limb, and left it in the bandage for six weeks, he found a little more angular deformity than he wished.

Mr. Mapother said since Mr. Corley mentioned the use of this apparatus to him he had tried it in club-foot, and in two cases it had been fairly successful. In another, on account of the extremely small size of the foot, it was not easily applied, but in two cases where the children were three or four years' old it suited admirably. After division of the tendo Achillis, he placed the foot in proper position, and then with this admirable apparatus he had completed extension, and the result was satisfactory. In another case he applied a roller covered with plaster-of-Paris, and covered with an ordinary bandage. Mr. Corley had called this the Bavarian apparatus. He was sure he had forgotten to state, as he did when he first mentioned the subject, that it was introduced to this kingdom by Asst.-Surgeon Moffett, a pupil of the College in 1855, and a licentiate. He had noticed it in use during the Franco-Prussian war, and had described it in his book on "Ambulance Work."

Mr. Stapleton observed that this was not what was called the plaster bandage. What was used in Berlin, where he had seen it used, was a thin material, which was dusted very thickly with plaster-of-Paris. It was then soaked in water, and rolled round the limb, The material used was very open, like what they in this country called a lint bandage.

Mr. Fleming said the apparatus which had been just described by Mr. Stapleton was the one which had been used here for a long time. He used it himself in the Richmond hospital, and if he was not mistaken, it was a German who introduced it to this country. He believed he was

a Professor in one of the principal schools, but he forgot his name. It consisted of a gauzy bandage of very open work, and was dusted over, he believed, with a preparation of plaster. It was then damped and applied to the limb, and plaster was rapidly applied with the hand to the outside. It quickly formed a tolerably solid case, but it took a long time to dry satisfactorily. Some of his cases turned out well, but in some of them there was considerable deformity. He thought they were much indebted to Mr. Corley for bringing this new apparatus before the Society. In the first place it was capable of extremely easy and rapid application; in the next it was of great advantage to the surgeon, because it enabled him to watch carefully the progress of the case; and if, as it did happen in injuries such as those of the ankle-joint, there was effusion in twenty-four hours or so after, it was of great advantage that they should have the opportunity of ascertaining whether such a condition existed, and of applying the necessary remedies. He thought hospital surgeons should be very glad to have the opportunity of putting this apparatus into use as often as they had the opportunity.

Mr. Richardson observed that many years ago M. Seutin, owing to the accidents likely to occur under the use of the immovable starch apparatus, such as excoriations and gangrene, substituted for it the starched apparatus having the properties of movability and immovability, and which he named the *Amovo-Inamovible* apparatus, the principle being identical with that just illustrated by Mr. Corley. In M. Seutin's starch apparatus the movability is imparted to it by dividing the apparatus, when dry, its whole length in front, and after some time should the limb shrink, he removes some of the edges of the apparatus to enable it to fit close to the limb and give it firm support. This apparatus has been compared by Goffre's in his "*Précis Iconographique de Bandages, Pansements et Appareils,*" to a bi-valve shell. Indeed, in Goffre's book the bi-valve term is also applied to the plaster apparatus constructed on this principle.

Mr. Stapleton said that an apparatus had to be used to cut it.

Mr. Richardson said that was another matter. Seutin had established the bi-valve principle, and this name is applied to the *Amovo-Inamovible* starch, and even to the plaster apparatus in Goffre's work. He (Mr. Richardson) had often made Seutin's *Amovo-Inamovible* apparatus, and found that after it had been removed and re-applied two or three times, the starch cracked along the middle line posteriorly, and allowed of its easy removal and re-application.

The President wished to know how Mr. Corley put up

Colles's fracture in this apparatus, because one of its advantages was its universal applicability.

Mr. Corley, in reply, said that with reference to Dr. Mapother's observation, he had stated in his previous communication that he did not claim the slightest originality in the matter. He got a description of the apparatus in one of the Army Medical Reports. It struck him that it would be a most convenient apparatus, and having tried it he found that it exceeded his most sanguine expectations. The history given by Mr. Moffett, who was a resident pupil at Jervis-street Hospital at the same time as Mr. Walsh, was in a communication made by him while at Netley, and he mentioned that it was communicated to him by Dr. Streidlinger, as the apparatus used in the Bavarian ambulances, but it was intended rather as a temporary than, as he understood Mr. Moffett's paper, a permanent arrangement. He (Mr. Corley) found on trial that there were few fractures to which it was not applicable. As to Mr. Richardson's observations about the bi-valve, he had only to say that in this case the line of stitching prevented the plaster on each side from meeting, so that a hinge was left like the back of a book.

Mr. Richardson observed that the term bi-valve had been long ago applied in French books to this apparatus.

Mr. Corley continued to say that the starch bandage took two or three days to harden. He remembered, as a student, attending an hospital where the "succession duty" plan was carried on by the surgeons, and he saw a fracture put up on one of the last couple of days of the month. It was not held in position, and the in-coming surgeon did not take the same interest in the case, and he remembered that the limb presented a very remarkable elbow on the external side. It impressed him very much with the importance of keeping fractures in position while the starch was hardening. That would take three days, while the apparatus, if the plaster was good, would be hard in three or four minutes. With regard to Colles's fracture, he held it securely in his hand in the ordinary way, and put a pad over the dorsal prominence, just where the splint recommended by Dr. Smith pressed. While so holding the fracture he grasped the outer layers, so as to press, and to secure the same object at the lower end, that was secured by the keel in Gordon's splint. The fracture in which he tried it was doing well. It had been put up in Gordon's splint. The other was put up in the ordinary pistol splint, and was giving the patient great pain and uneasiness. He put the fracture up in this apparatus, and the man was able to get up next day, and never had the least pain afterwards.—*Medical Press and Circular*, *Feb.* 14, 1872, *p.* 136.

## 44.—FRACTURE OF THIGH-BONE, AND ITS TREATMENT WITH THE AMERICAN APPARATUS.

By B. Wills Richardson, Esq., Surgeon to the Adelaide Hospital, Dublin.

Of the numerous appliances that have been invented for the treatment of fractured thigh-bone there are few more capable of producing good results, with little trouble to the surgeon, than the one known to European practitioners by the name of the "American" apparatus. When it is used the thigh need not be bandaged—a most important matter in oblique fracture with overlapping fragments, for many of the muscles of the thigh are so disposed that the constriction exercised by the bandage becomes itself a cause of shortening. Indeed, the muscles of the internal and posterior part of the thigh have been compared to cords extended between their osseous attachments. The roller necessarily presses these muscle cords towards the femur, and in this way, by acting antagonistic to the extending and counter-extending forces, tends to approximate their origins and insertions, and to draw the lower fragment of the femur towards the pelvis.

It is remarkable that Nélaton, the countryman of Desault and Boyer, whose splints have contributed to their fame, prefers the American apparatus to the splints of those celebrated men, as well as to every other apparatus known to him, for the treatment of oblique fracture of the shaft of the femur.

The mechanism for carrying out the American method of treating fracture of the shaft of the thigh bone varies a good deal, as will be seen in Hamilton's work on "Fractures and Dislocations." I have found that Dr. Hartshorne's apparatus, with some modifications of my own, thoroughly counteracts the tendency to overlapping of the fragments. Its cost, moreover, is very trifling.

Dr. Hartshorne uses two splints, and dispenses with the perineal band, which I retain.

In addition to the transverse bar which carries the extending screw, his apparatus has a second bar that is connected with a foot-piece to which the foot is secured. Dr. Chapin uses two separate extending screws; but one screw with a double hook acts better. His apparatus is then constituted as follows:—

1. An external splint (pine wood) of sufficient length to extend from the axilla to some inches below the foot. Its width should be such that the holes for the ends of the perineal band may not be too near one another. The holes should also be only a short distance from the upper end of the splint that the anterior portion of the perineal band may not press the groin, and favour swelling of the limb. The splint is secured

to the trunk by means of a couple of saddle girth bands, each provided with leather straps and buckles, and to the short splint by three leather straps with buckles, as well as by the transverse bar. The upper end of the splint should have a cushioned cap.

2. A short internal splint (pine wood) to extend from the perineum to the same distance below the foot as the external splint. Its upper end should likewise have a cushioned cap. Some of the American apparatuses have no inside splint, in which case the transverse bar should be immovably fixed to the outer splint. I use, however, the inner splint, because it prevents the long one from shifting its position, and thus assists in rendering the apparatus as firm and as steady as a box splint.

3. A birch-wood rectangular bar of the following dimensions:—Length, 12 inches; width from before backwards, 2 inches; vertical depth, $1\frac{1}{2}$ inches. From four to six holes are drilled in it at a distance from one another a little more than the width of the substance of the splints. They are for birch-wood pegs, which secure the parallelism of the lateral splints, and maintain them at the requisite distance from one another. Although not represented of this proportion in the illustration the bar should have the width from before backwards I have mentioned, to enable the male extending screw to turn steadily in it. This screw, including its ring handle, should be at least 14 inches long.

4. A female screw (iron) which is securely fixed in the transverse bar.

5. In the apparatus I used first there was but one swivel hook attached to the extending screw. Two hooks either soldered or riveted to a single plate, as seen in the figure, are preferable. Each of these hooks should be separated from one another by a space a little greater than the width of the ankle at the malleoli. The hooks form one piece with a narrow transverse iron plate, from the ends of which they rise, and which is secured to the extending screw by means of a swivel joint. It will be seen by the illustration that each side of the adhesive plaster loop diverges from the malleolus to its corresponding hook; therefore, when the extending screw is acting, the sides of the loop are prevented from unduly pressing the skin over the malleoli. On the other hand, should but one hook be used, the sides of the loop must converge to it, so that the greater the extension made with the screw, the greater the compression of the soft parts covering those processes, and the greater the risk of sloughing. To prevent this, wadding must be freely used under the strapping and a piece of wood broader than the sole of the foot secured between the sides of the loop. The double hook obviates the necessity of these precautions, with the ex-

ception of the use of wadding. This, however, is not to be used to an amount that would render the area of the adhesion of the strapping to the leg not sufficiently extensive to enable the strapping to resist the strain to which it is submitted by the action of the extending screw.

6. It is scarcely necessary to observe that a properly covered and stuffed perineal band, suitable pads, wadding, and scored splint form part of the apparatus.

*Mode of applying the Apparatus.*—Although the method of applying the adhesive plaster strapping and loop to the leg is described in Hamilton's, and in many other works which treat of fractures, I will, nevertheless, explain it here for the convenience of the junior reader.

The limb having been sponged with vinegar diluted wfth water and dried, the hollows of the leg are filled with wadding. Five superimposed straps of soap plaster, having a width of two inches, and sufficient length to form an eight-inch loop below the sole of the foot, are then applied to its sides. The ends of these straps should be placed at corresponding points a little above the knee at each side. The straps are secured to the leg with straps of soap plaster, according to the ordinary way of strapping.

The leg thus prepared, and the perineal band placed *in situ*, the saddle-girth straps are laid under the body, and the leather ones under the fractured limb, with the intervention of a pad long enough to extend from the buttock to the heel. The lateral splints are now applied, either wadding or pads being interposed between them and the soft parts. The transverse bar is at the same time secured by the pegs in the square openings made for it in the splints. The latter should pass a sufficient distance beyond the sole of the foot that the extending screw may be screwed home to its ring handle before it can act upon the loop. If this precaution be not taken, longer splints may subsequently be required, unless the plaster loop can be sufficiently shortened to enable the extending screw to act upon the limb. In hospital practice having to change the first applied splints for longer ones would be of little moment, but in private practice such evidence of miscalculation had better be avoided.

The ends of the perineal band when passed through the holes at the axillary end of the long splint are then tied. All the straps are next buckled.

When necessary, a scored splint and an underlying pad are to be secured on the front of the thigh by a couple of straps.

Unless the case be such as to require previous co-aptation of the fragments, this may be left to the influence of the extending screw, the hooks of which are passed into the plaster loop, the screw turned, and extension gradually effected.

A few turns of the screw morning and evening, for a couple of days, cause the patient but little annoyance, the muscles appearing to offer less resistance when thus overcome than by a more rapid extension.

The following advantages may be claimed for this modified American apparatus:—

1. Facility of application.
2. Limb can be accurately measured without being disturbed.
3. Traction is made in the natural axis of the limb.
4. There being no bandages to become loose, it rarely requires re-adjustment after the first few days. It is therefore admirably suited for provincial practice, when, as too often happens, long distances have to be travelled, and visits at short intervals may be impossible.
5. Whenever the fragments are capable of being united without shortening, it is thoroughly efficient in producing this result. Speaking of the American apparatus, Nélaton says, that it has over Desault's, Boyer's, Bonnet's, Velpeau's, and Gariel's splints such a superiority, that he has cured without shortening all fractures of the body of the femur, for the treatment of which he has applied it, either in hospital or in town practice.
6. The expense of the apparatus is but trifling, the straps, transverse bar, and extending screw, costing only a few shillings. —*Dublin Quarterly Journal, Nov.* 1871, *p.* 285.

---

## 45.—ON EXCISION OF THE HIP-JOINT.

By HENRY HANCOCK, Esq., Senior Surgeon to Charing-cross Hospital.

[The objections to the performance of this operation, which are most usually urged, are as follows:]

If the acetabulum is carious, the patient, whether operated upon or not, must die.

When caries attacks the surfaces of a joint, it is never limited to one of the bones which compose it; therefore, as the acetabulum does not admit of removal, the operation is useless, as it does not get rid of the whole of the disease.

That the operation is bloody and formidable.

That, to be successful, the head of the bone must have been previously dislocated.

That at the time of operation there must be no, or at all events very little, disease of the acetabulum.

That there should be no communication between the outer abscess and the pelvis.

That the disease must be in its last stage.

Whilst, lastly, it is objected that the disease may possibly be mistaken for one of the sacro-iliac synchondrosis or of the pelvis itself; and that, moreover, the operation is useless, as death in hip disease does not occur from local mischief, but from the exhaustion resulting from the constitutional malady.

Now, in the first place, Is the acetabulum always implicated or not? It may be in a very large majority of cases: it certainly is not so in all. Of eighty-one cases in which the fact is specially alluded to, I find that in eighteen, or rather more than one-fifth, the acetabulum is expressly stated to have been healthy.

Although these exceptions are too numerous to prove a general rule, they are by no means sufficient to divest disease of the hip-joint of its gravity and fatal character. If it be true that he who suffers from caries of the acetabulum *must die*, caries of the hip-joint must take its place amongst the most destructive and malignant diseases, even rivalling in its fatal character cholera, typhus fever, small-pox, &c., since for four-fifths of their number there is no hope, they must inevitably die. But is this actually the case? Is caries of the acetabulum of so peculiar a character that it is not amenable to the same laws as govern that disease elsewhere? Is the cotyloid cavity so entirely without the pale of vitality and so wanting in reparative power that when once attacked with caries it cannot be cured, and the patient must inevitably die?

This seems to me to be an extremely difficult position either to maintain or to prove.

There are many who assert that these cases, if left to nature, rest, and proper nourishment, will do well of themselves. There are many of these cases which recover without operative interference, and of which post-mortem examinations are never obtained. There are patients, again, who pass through the successive stages of the disease—inflammation, ulceration, suppuration, spontaneous luxation, flattening and absorption of the head and neck of the femur—and yet who preserve their lives at the expense of the joint. True it is that in these cases the acetabulum as an acetabulum is not cured—it is obliterated; it undergoes the double process of disintegration and filling up; it becomes, in fact, in process of time, a thing of the past. But the patient does not die; for whilst his life has been endangered by the progressive destruction of the joint, it has now been saved by completion of that destruction and the subsequent reparation and reproduction of bone and soft parts.

As illustrative of this fact take the case operated upon by Mr. Anthony White, in which there was not a vestige of the acetabulum remaining; take also the following case, operated upon by Sir William Fergusson in 1849:—E. N., aged 10 years,

two years and a half before had been knocked down by another girl, struck her left hip, and was at once lamed by the accident. In process of time abcesses burst around the joint, and she suffered much from pain and swelling extending from the knee along the back of the thigh. Twelve months after the disease began, the parents one morning observed that her thigh was altogether flexed across the abdomen, and that she was unable to move it from this position, Fresh abscesses occurred, and the suppuration with the constant pain destroyed her health, and she became weak and emaciated to the last degree. After remaining at a water establishment for six months she was taken to King's College Hospital. The left buttock was unnaturally prominent, smooth, and tense, and over the back of the ilium was a hard globular swelling, evidently the head of the femur dislocated upwards. Around this were various sinuses burrowing in all directions, and communicating with the carious head of the femur. Some of these sores were situated in the groin, some over the sacrum. A very large continuous one extended down the thigh, and they all discharged much unhealthy pus. The slightest movement or pressure caused her great pain. She lay on the sound side of her body, with the knee of her diseased limb thrown over the opposite thigh.

On January 11 Sir William Fergusson enlarged one of the sinuses, and removed a portion of dead bone which was loose. The head of the bone was found in a carious condition, and he consequently removed it on January 13. The head of the bone was much reduced in size, and carious. There was hardly any trace of the cotyloid cavity, it being filled up with new bone.

Do not these and analogous cases also offer a very cogent reply to the objection that the operation must have originated and been conducted in forgetfulness of the well-know pathological fact that when caries attacks the surface of a joint it is never limited to one of the bones which compose the articulation, and that as the acetabulum cannot be removed with any prospect of advantage in the living body, therefore excision of the head of the thigh-bone for caries of the joint should be regarded as no less erroneous in theory than objectionable in practice?

It seems to me that, so far from this being the fact—so far from regarding it as either one or the other—by performing excision of the head of the thigh-bone in caries of the joint we emulate the proceedings of nature, and assist those whose powers of constitution are too weak, and who are consequently unable to carry those proceedings to completion.

Mr. Rankin, of Carluke, has an illustrative specimen in his possession. "The head of the femur in this case was discharged

spontaneously, and bears the brand of what has long been called caries; yet after many years the person is alive and healthy, with a free joint over the pelvis, though the knee is flexed and anchylosed." According to Mr. Burford Norman, the late Mr. Liston possessed the head of a femur which had been detached and spontaneously expelled; and on the table before you is a similar preparation spontaneously expelled from a patient of Mr. Gant, to whose kindness I am indebted for the opportunity of exhibiting it to you. How frequently, also, has the head of the femur, so separated, been removed by operation. We have already seen that the very first removal of the head of the thigh-bone from the acetabulum was a case of this description performed by Schmalz in 1816. In 1730 John Daniel Schlichting dilated a fistulous opening over the hip of a young girl aged 14, long the subject of hip disease, and through it extracted the head of the femur, which had become detached. Similar cases are recorded by Vogel, Harris, Reid, &c.

Watch a patient suffering from scrofulous disease of the hip-joint: for many months the symptoms may consist only of stiffness, tenderness, and more or less weakness and limping, without hectic or constitutional disturbance of any kind. Watch the same patient during the progress of ulceration, suppuration, destruction of the ligamentum teres, and consequent dislocation: then the hectic and constitutional irritation are at their height. Watch the same patient again when dislocation has completely obtained: the disease now appears to have exhausted its rage, to have overcome all obstacles to its course, and to be satisfied with the mischief it has caused; the constitutional symptoms subside, the abscesses frequently heal up, and a cure takes place.

Can it be asserted that a case such as this is not one of caries of the joint? Can it be also asserted that the acetabulum is not here affected? that the fact of a cure is conclusive against such a proposition?

It would seem that in such instances the luxation of the head of the bone is essential to recovery; and when it does take place spontaneously, other symptoms being favourable, I would not urge too great haste in proposing an operation. But, alas! how frequently is it otherwise; how frequently does the strength of the patient prove insufficient to cope with the disease. The bone becomes carious, rough, or broken up; sinuses form, they continue to discharge, and still further to weaken the patient; hectic fever, night sweats, emaciation, loss of appetite, restlessness, cough, in some cases, spitting of blood, ensue; the lungs become affected, and the patient dies. It is in such cases as these, when the irritable and frequent pulse,

the hectic flush, the wasting strength, the anxious and suffering countenance too surely denote the undue severity of the trial and the too great probability of death, that the Surgeon should throw aside his doubts, should boldly confront the mischief, and, by imitating the wise and beneficient workings of nature, endeavour to snatch his patient from that destruction which otherwise inevitably awaits him. We have seen what takes place in the acetabulum when spontaneous dislocation of the head of the bone has taken place. What, I should like to ask, is there in the operation to prevent the same process taking place after the head of the bone has been removed artificially?

But if these objections have had their influence, and been allowed to prevail as they undoubtedly have done, what a signal triumph must excision of the hip-joint have achieved. since we find that, of 126 patients, suffering from caries, who according to these theories must inevitably have died, no less than seventy-one recovered with more or less useful limbs!

Again, we are told that the operation for removal of the head of the thigh-bone should be restricted to those cases in which the head of the bone is dislocated. Although this was a point mainly insisted upon by those who advocated the operation prior to the year 1857, no one attempted to assign a reason for the restriction save Mr. Coulson, who says—"The first condition is the absolute displacement of the head of the bone; for as long as the neck and the trochanter are in relation to the acetabulum there is a chance of anchylosis taking place, which is the most favourable termination to be looked for."

This appears to me to be a scarcely tenable position; it has never as yet been proposed to operate whilst a chance of cure by other means has been apparent. Anchylosis, it must be remembered, is the last process of hip disease; it must have been preceded by the inflammation, ulceration, and suppuration, and by the hectic fever, constitutional irritation, and consequent weakness and emaciation. It is by no means all who die of hip-joint disease who have attained the stage of anchylosis.

We know that whilst in some cases the head and neck of the femur may be destroyed by caries, in some they are comparatively sound; and in others, again, both they and the acetabulum are free from disease, the patients dying from the intensity of the constitutional irritation rather than from the amount of local mischief. Take the following case, related by Sir Benjamin Brodie:—A young lady, aged 9 years, being at play on Jan. 1, 1808, fell, and wrenched her hip. She experienced so little uneasiness, that she walked out that day as usual. In the evening she went to a dance, but while there was seized with a rigor, was carried home, and put to bed.

Next morning she was much indisposed, and complained of pain in the thigh and knee. On the following day she had pain in the hip, and was very feverish. These symptoms continued; she became delirious, and died just a week from the time of the accident.

On inspecting the body on the following day, the viscera of the thorax and abdomen were found perfectly healthy. The hip-joint on the side of the injury contained about half an ounce of dark-coloured pus, and the synovial membrane, where it was reflected over the neck of the femur, was destroyed by ulceration for about the extent of a shilling.

In another case, the preparation of which was shown to the Medical Society of London by Mr. W. Smith, the patient, a boy in the employ of one of the railway companies, was delivering parcels on a Christmas morning. In getting out of the waggon he slipped, and sprained his hip. He thought nothing of it at the time, but after a few hours he became so ill that he was obliged to be taken home. The subsequent progress of the symptoms closely resembled those in Sir Benjamin Brodie's case, and the boy died within a week after the accident. The hip-joint after death was found to contain about a tablespoonful of decomposed blood and matter, and the synovial membrane presented a rent about three-quarters of an inch long at the point where it was reflected over the neck of the femur.

Fortunately, such cases as these are rare, but I think that in the present day few Surgeons would fail, when the delirium came on, to recognise the gravity of the case, or hesitate for one moment to excise the head of the femur, and thus get rid of the poisonous fetid discharge and its source, and at the same time afford the patient a chance of recovery, since the conformation of the joint is such as to render a mere opening into the capsular ligament insufficient for the evacuation of the pent-up discharge.

We know, also, that the head of the bone may be entirely detached from the neck, and still remain in the acetabulum; and, lastly, that caries and anchylosis may coexist in the same joint, and the patient sink worn out before the latter is completed notwithstanding the head and neck of the femur have remained in relation with the acetabulum.

It is possible that the dread of exposing so large a joint-surface as the acetabulum, and of the supposed consequent tax upon the patient's power of endurance in getting rid of the joint structures and the obliteration of the cavity, may have influenced this recommendation; but experience has proved that the danger of exposing joint-surfaces is for the most part inverse to the extent of surface exposed, whilst it has also shown that in this operation the presence of the investing

cartilage exerts but comparatively slight influence over the healing of the wound.

The stress laid by Mr. Anthony White upon the displaced head of the bone acting as a foreign body, and also upon the complete obliteration of the acetabulum, with the successful termination of that case, doubtless had its weight; but it is curious that in Schmalz's case—the first, as we have seen, on record—both conditions obtained. The head of the left femur was dislocated on to the dorsum ilii. That of the right was still in its cavity, which, however, was in a state of suppuration. It was the latter which Schmalz excised, and the patient did well.

*A priori*, it is reasonable to suppose that when the head of the bone has been dislocated for sufficient time for the acetabulum to be obliterated, the operation would offer a greater chance of success than when performed under other circumstances; that, in point of fact, the process of healing would be simplified and shortened. But against this must be placed the intense struggle of that fearful period preceding dislocation, which tries the patient's powers of endurance to the utmost limit; and it should, moreover, be remembered that the value of this recommendation, if any, exists, not in the mere displacement of the head of the bone, but in the obliteration, cure, or absence of disease, of the cotyloid cavity. And this coincides for the most part what is proved by the statistics of the operation. Sixteen operations were performed in which spontaneous luxation had taken place, and wherein the acetabulum was found to be either entirely obliterated or in a healthy condition; of these 8 were cured, 2 died, and the results of 6 are stated to have been doubtful. Forty-three operations were performed in which spontaneous luxation had taken place, but wherein the acetabula were found diseased, and in many instances were gouged, in some few cauterised; of these 43 operations, 16 resulted in cure, 18 died, and the results of 9 were doubtful. Forty-four operations were performed in which the head of the bone was found in the acetabulum, but in which the latter was implicated in the disease; of these, 28 were cured, 10 died, and the results of 6 were doubtful. Eight operations were performed in which the head of the bone was found in the acetabulum, the latter being healthy; of these, 3 recovered, 4 died, 1 was doubtful. Ten operations were performed in which the head of the bone was found absorbed and the acetabulum diseased; of these, 5 were cured, and 5 died.

Hence we find that, had the recommendation that the operation should be restricted to those cases in which spontaneous luxation had already taken place prevailed, the sphere of its utility would have been very much and very prejudicially con-

tracted, since of 123 cases, 52 were performed in which the head of the femur remained in the cotyloid cavity.

It is sometimes extremely difficult, if not almost impossible, to tell with any degree of certainty prior to the operation whether the head of the bone be dislocated or not. And after all, the success of the operation would appear to depend very much less upon this point than those who made this recommendation seem to have supposed; for we see that in the 16 cases where the head of the bone was dislocated and the acetabulum free from disease, the mortality was 12 per cent. In the 43 cases where the bone was dislocated and the acetabulum diseased, the mortality was 56 per cent. In the 10 cases where the head of the bone was absorbed and the acetabulum diseased, the mortality was 50 per cent. In the 44 cases where the head of the bone remained in the cotyloid cavity which was diseased, the mortality was 28 per cent.; whilst in the 8 cases in which the head of the bone remained in the cotyloid cavity, the latter being healthy, the mortality is by far the highest, being 62 per cent.—*Med. Times and Gazette, March* 16, 1872, *p.* 305.

---

## 46.—DR. BIGELOW'S VIEWS ON DISLOCATION OF THE HIP-JOINT.

By JONATHAN HUTCHINSON, Esq., Senior Surgeon to the London Hospital, and Lecturer on Surgery.

[Dr. Bigelow, of Boston, U. S., has lately published an essay on the Mechanism of Dislocation and Fracture of the Hip. Mr. Hutchinson recommends this essay strongly, as being an excellent example of experimental investigation applied to a subject concerning which but too many have been contented merely to speculate.]

The main fact to which I must ask your close attention is, that the ilio-femoral ligament is the strongest part of the envelopes of the joint; that it is very rarely torn through; and that it is to it that the peculiar positions of the bone in the several displacements are chiefly due. This ligament must be described as arising from the anterior inferior spine of the ilium, passing downwards and outwards, and being inserted into the whole length of the anterior intertrochanteric line. Thus at its origin it is not more than half an inch broad, and at its insertion more than two inches. It is supplementary to the capsular ligament, over which it passes, and with which it is inseparably connected. You will find in most anatomical works that the term ilio-femoral or "accessory intertrochanteric ligament" is restricted to the lower part of the structure described; but as the whole arises from one part, there can, I

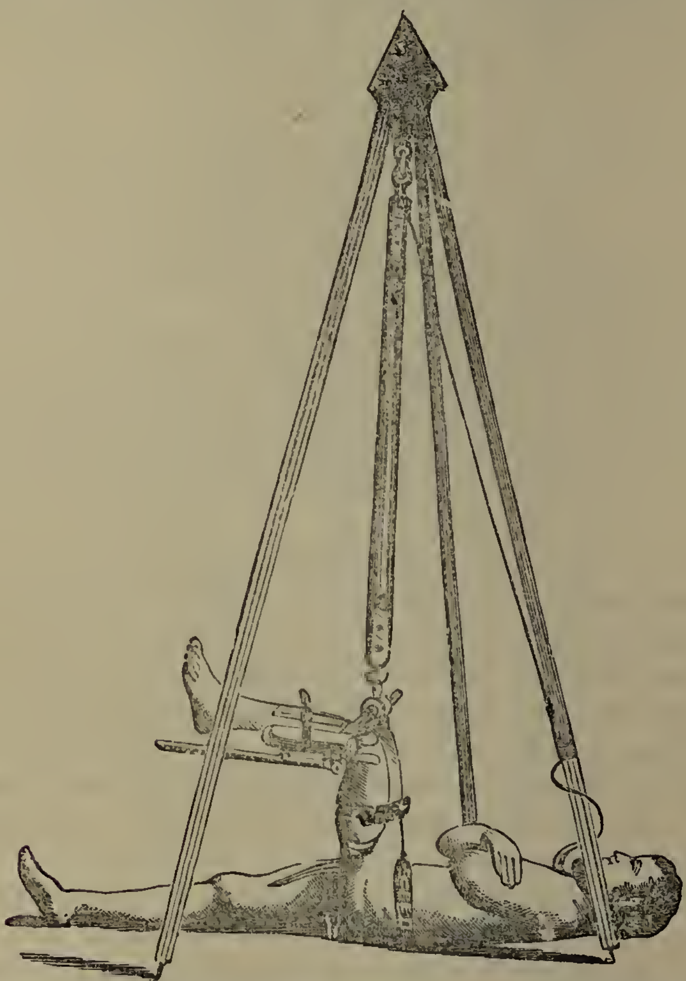
think, be no question that it is well to regard it in its entirety. Dr. Bigelow purposes for it the name of the Y-ligament, because its lower fan-shaped expansion has a space between its upper and lower portions like that between the arms of an inverted Y. For myself, I doubt much whether anything is gained by rechristening it. However, whatever name we give it, the truth remains the same, that it is this ligament—as strong as the ligamentum patellæ or the tendo Achillis—which in dislocations restrains the movements of the great trochanter, and thus regulates the position of the limb. It is this ligament which, when we reduce by manipulation, guides the movements of the bone. We have long known that the ligamentum teres has nothing to do with these matters, being indeed powerless to keep the bone in the socket; and we may now, I think, discard, to a large extent, the supposed influence of muscles.

In the common form of dorsal or posterior dislocations, the Y-ligament holds the great trochanter to the pelvis, and thus inverts the limb. It also, assisted by other lower fibres of the capsular, hinders the passage of the head of the bone upwards, and thus keeps the shortening within limits. Upon it, as upon a pivot, the neck of the bone will move, if you commence manipulations and use the shaft as a lever. It will be by its aid, and not, as erroneously supposed, by that of the adductor muscles, that the head will be directed into the socket if you flex and then abduct (or, still better, circumduct outwards,), as in our usual method of reduction.

Amongst the special services which Dr. Bigelow has done, is that of explaining the peculiarity of "the dislocations into the ischiatic notch," and of teaching us how to reduce them. His assertion is, that they differ very little from the majority of so-called dorsal dislocations, for in both the head of the bone lies not so much on the dorsum ilii as just behind, or behind and above, the acetabulum; and in neither does it really pass so far back as the sciatic notch. The real difference between the two consists in the relation of the head and neck of the bone to the tendon of the internal obturator muscle and to the portion of capsular ligament beneath it, these being below the neck in a true dorsal, and above it in the so-called ischiatic dislocation. This fully explains the increased difficulty in reducing the latter dislocations by the old method, and also the ease with which the flexion plan succeeds.

Dr. Bigelow believes that the greater or less laceration of the capsular ligament is a matter of very little consequence to the patient's chance of good recovery; and he instructs us how, in cases when the smallness of the hole in this structure is supposed to be the impediment to reduction, we can easily and safely tear it as much as we wish.

It is not my intention to bring before you in detail at the present time the important statements made by the author I am quoting respecting the other special forms of dislocation. These may be better reserved until we have an opportunity for demonstrating the dislocations on the dead body. It will be sufficient at present to say that they seem to me to simplify greatly a very difficult subject, and that I have no doubt that they will speedily bear good fruit at the bedside. What I chiefly wish you to recollect is, that it is the strong ilio-femoral ligament which in the backward or dorsal dislocations, holds the great trochanter forwards and inverts the limb, which in the dislocations downwards, still holding the trochanter, flexes the limb, and in dislocations of the head of the bone inwards (thyroid), still restraining the movements of the great trochanter, everts the limb. All these results can be easily witnessed by experiment in the dead body. Lastly, in some rare cases in which the displacement follows no rule, the explanation is to be



Dr. Bigelow's Tripod for angular extension. The pelvis is buckled to the floor. The flexed leg is suspended by pulleys from the apex of the tripod. A bar fixed to the splints, and crossing the knees transversely, serves to rotate the limb.

sought in the fact that this ligament has been wholly or partially torn through. It is, however, very strong, and its rupture is very infrequent.

I need scarcely add that Dr. Bigelow is of course a warm advocate of the manipulation, or flexion, method of reduction. He urges that almost anything may be done if you only recollect the manner in which the ilio-femoral ligament will be stretched in the different positions of the bone. Extension horizontally, whether in a straight line or obliquely, he for the most part opposes strongly, as only likely to be successful after forcibly tearing the important structure referred to. Extension with the thigh bent at right angles to the pelvis he has warmly advocates, for in this position the ligament is relaxed. For the accomplishment of this he has devised a special apparatus, which looks likely to be very useful. As a practical suggestion likely to be available under the frequent conditions when this triangle may not be at hand, he advises that the surgeon should place his foot (unbooted) on the patient's pelvis, and then lift from the knee.—*British Medical Journal, Dec.* 23, 1872, *p.* 718.

---

## 47.—THREE CASES OF WOUNDS OF JOINTS.

Under the care of Mr. Hey and Mr. Jessop, at the Leeds General Infirmary.

We are very glad to bring before the notice of our readers the three following cases of injuries to joints which have all terminated favourably. They will be read with great interest by all who perused Mr. Couper's case of injury to the knee-joint which we reported a week or two since. It will be seen that the antiseptic treatment was strictly followed only in the first two cases, but that the result was equally favourable in all of them. The first case was undoubtedly the gravest. Injuries to joints, except as part of very severe injuries, are not common, and we here repeat that we shall be glad to receive details of any similar cases.

We are indebted for the notes of these cases to Mr. McGill, the resident medical officer.

*Case* 1.—*Wound of the ankle-joint.*—J. B., aged thirty-five, was admitted under the care of Mr. Hey on Aug. 2nd. There was a wound one inch long situated over the left internal malleolus; through this wound it could be felt that the malleolus was broken, and that the ankle-joint was opened, the finger passing between the tibia and the astragalus. There was also a small wound on the anterior part of the ankle which did not communicate with the joint. The accident was caused by a block of stone falling on to his leg.

The joint was syringed out with carbolic lotion (1 to 20) and the antiseptic dressing applied with all care in the usual manner. The leg was placed on a swing cage. The antiseptic dressing was continued for a fortnight, by which time the wounds were healed without suppuration. On the evening of the first day the temperature was 100°; it was never any higher; and was normal by the fourth day. The splint was kept on for a month. The patient was discharged with the leg in a plaster-of-Paris bandage on Sept. 5th. He is now walking about with a perfectly movable joint.

*Case* 2.—*Wound of the knee-joint.*—J. S., aged thirty-one, was admitted July 2nd under the care of Mr. Jessop, with a wound four inches long across the thigh, one inch above the patella; through this the finger could be passed into the joint, and the posterior surface of the patella plainly felt. Round the lower and inner sides of the patella were three other wounds, each about one inch long; no communication of these with the joint was made out. The injury was caused by the patient falling with his knee bent through an earthenware pan.

A back splint was applied. The antiseptic dressing was applied, but the joint was *not* syringed out with carbolic lotion. The antiseptic dressing was continued for eleven days, by which time all the deep part of the wound was healed, and a small superficial granulating surface was left. Twelve hours after the accident the temperature was at its highest, 99·8°; it became normal by the fifth day. The pulse was never above 92. On July 30th the splint was removed. The patient was discharged a few days after; he now walks with a movable joint, without any limp.

*Case* 3.—*Wound of the knee-joint.*—P. M., aged twenty-nine, admitted Jan. 26th, under the care of Mr. Hey, with a small perforating wound below and to the outer side of the patella, through which synovia was flowing. A probe could be passed up into the joint through the opening. The wound was caused by the man falling three feet on to a sharp-pointed iron rod belonging to some machinery in a flax-mill.

A back splint was applied, and the wound left uncovered. After twelve hours, as the discharge of synovia still continued, carbolic acid and glycerine was applied, and was changed daily for a week; no precautions, such as are used in the antiseptic dressing of wounds, were observed. The wound healed entirely in two days. The temperature reached 100·2° on the third day, when there was some effusion in the joint, but became normal two days after. Now (February 11th) the effusion is all gone, the splint has just been removed, and the movements of the joint are perfect. The patient will probably be discharged in another week.—*Lancet, Feb.* 24, 1872, *p.* 256.

## 48.—ON AMPUTATION FOR DISEASE OF THE KNEE-JOINT.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

[In the first part of this paper the writer describes four cases of amputation for knee-joint disease, and then passes on to one or two points in the operation.]

You will have observed that, except in one instance, where disease of the soft parts prevented its application, two lateral skin flaps were made in amputating, and then a circular section of the muscles to the bone. My colleague, Mr. Poland, as you saw the other day, performs the ordinary flap operation. It is impossible, and indeed unnecessary, to institute any comparison between the two; either procedure would have been equally suitable, as far as the mere amputation was concerned, in all my cases. I prefer, however, my way because we practise torsion here; and I believe on this plan the twisting of the vessels is rendered much more easy of accomplishment. In muscle-flaps the vessels are *slit* often for some little way along their channel, and so torsion becomes a little difficult in its application. Remember this in practice; for no student leaving Guy's Hospital will, I apprehend, ever use, except, may be, in very exceptional instances, any other method of arresting hemorrhage than this. I sometimes fear, indeed, that some of you may not know how to apply a ligature to an artery on the living subject.

But I have other objections to the flap operation as usually performed; and one of these you saw during the last amputation. I had to cut out a piece of the sciatic nerve from the flap. This is a proceeding first, I think, suggested by Mr. Hilton; and it is done with the idea of obviating any liability to what is called "irritable or painful stump." All nerves, when they are cut across and the divided ends separated, become what is styled bulbous—that is to say, new tissue is thrown out in and around their sheaths from inflammatory action; therefore, in this respect, the circular and high division of the muscles would be no better than the other. But in the ordinary amputation, unless the nerve be cut across a second time, and dissected out of the flap—a method which I rather object to,—the bulbous ends become involved either in cicatricial tissue, and thus are pressed upon and made painful, or they are bound down at the extreme end of the stump, and become liable to pressure from various causes. A circular-division of the muscles at the bottom of the wound allows the nerves to become swollen out of harm's way; and therefore they produce then no symptom.

We have been remarkably free from such after ill-effects as irritable stumps in this hospital, and I attribute it entirely to

the cutting away of the nerve or making a high section of it. When, on the contrary, this is not looked to, perhaps in a few weeks, or it may be months or years, according as the scar-tissue contracts rapidly or the reverse, or pressure is brought to bear upon the stump soon or late, the patient will return to you complaining of agonising pain, and with tonic contraction of the muscles of the limb as its result, and oftentimes with some indolent ulceration about the cicatrix. The only treatment then available will be to submit the sufferer to another operation, opening up the stump and dissecting out the offending nerves.

In all these cases torsion was used, and in one of them—the first case in four years—some bleeding occurred a few hours after the operation ; possibly this may have come from a twisted vessel, but I do not think so, and for the following reason. We have been using lately a fine spray of carbolic-acid lotion directed over the stump during the twisting of the vessels, and I have frequently noticed, and my dressers with me, that the backs of our hands, over which the spray plays in common with the flaps, have been quite benumbed at the termination of the operation ; and if this be so with regard to parts covered with skin, much more so, I apprehend, will it be the case when the spray is brought into contact with the exposed muscle and vessels. It thus becomes easy to see that very probably some small vessels contracted at the time of the operation and ceased to bleed ; but when the parts became warm again, and the muscular fibre relaxed, then blood could flow a second time from their open ends by simply pushing away by the force of its current any slight clot that might have formed during the period of closure. If you accept this explanation of the bleeding in this case, you will be ready to admit that any like instance with it must not be used as a fact against torsion by the supporters of any other custom. If the hemorrhage occurs as the result of overlooking a vessel, it is quite certain that, surrounding conditions, such as cold or spray, being the same, the oversight would be just as likely to occur with one method as another. You must separate in your minds an occurrence like this, which is only a return of bleeding from temporarily closed vessels, within a few hours of, and the immediate consequence of the operation, from cases of true *secondary* hemorrhage, such as occur after ligature at the time of its separation, or possibly shortly before. An instance such as this has not occurred to me once during the time that I have practised torsion, now some four years and more.

In one or two of our cases we had a little difficulty in arresting the bleeding at the time of the operation by its means ; but this a matter depending partly on the number of vessels dis-

charging blood, and partly on the general health of the patient. With respect to the former element, in Case 4, it so happened that only one vessel required twisting, that one being the femoral; and a like absence of small vessels in the flaps has occurred to me once before. On the other hand, if the powers of the patient be feeble, there is often a trouble arising more from a general oozing from the whole surface than from any definite point that can be seized and closed.

Mr. Lister has discarded torsion, and now uses gut ligatures carbolised, which are cut off short and left in the wound. It is supposed that by some means they become absorbed, or possibly even revitalised by blending with the lymph and new tissue which unite the flaps. I must confess, however, that I do not feel inclined to abandon torsion; for a method that can be applied for four years to all cases of hemorrhage from wounds that have come under notice indiscriminately, which is quite free from any necessity of selection of fit and proper cases in order to ensure its success, and which, moreover, has never to my knowledge during that period been followed by *secondary* bleeding, requires no advocacy by word of mouth. The fact need only be stated to demand its trial by those who have not yet attempted it, and to ensure a continuance of the method by those who have commenced its use.

You will have noticed that I now perform all amputations under the antiseptic method with carbolic acid and all its paraphernalia. Last year I had eleven amputations of the thigh—all done without any such precautions; now all are performed with them. I hope by this means to arrive in a year or two at some definite conclusion on the subject, and to form some rule for future practice. Possibly, if hemorrhage again takes place soon after the operation, we may have to abandon the spray for some other means of disinfecting the surrounding atmosphere.—*Lancet, March* 30, 1872, *p.* 425.

---

## 49.—ON THE INSTRUMENTS DESIGNED FOR EXPLORING GUNSHOT WOUNDS.

By T. Longmore, Esq., C.B., Professor of Military Surgery at the Army Medical School, Netley.

[Whenever a gunshot wound is large enough to admit of the insertion of the Surgeon's finger, this alone should be used for exploring purposes. No artificial instrument can give the same amount of information. If necessary, the orifice of the wound may be enlarged by incision, for this is preferable to using undue force in introducing the finger.]

If the finger be not sufficiently long to reach the bottom of

the wound, even when the soft parts have been approximated by pressure from an opposite direction, and when the lodgment of a projectile is still suspected, or some other point of doubt remains to be solved, such as the direction the projectile has taken in the latter part of its course, we are compelled to make a further exploration by other means. Generally, a long silver probe, that can be bent if required, and that can be guided into a definite direction at the will of the surgeon, will be found to be the best substitute for the finger. The probe should be used with great discretion, for, without care, it may readily be made to inflict injury on vessels or on other structures which have escaped from direct contact with the ball, but have returned by their elasticity to the situations from which they had been pushed or drawn aside during its passage. It seems hardly necessary to observe that these directions for examining gun-shot wounds apply only to such as penetrate the extremities or extend superficially in other parts of the body; where a missile has entered any of the important cavities, such exploration would obviously be as useless as it would be mischievous.

In the majority of cases of gun-shot wounds there will be no difficulty in detecting the lodgment of foreign bodies, especially heavy ones, as bullets and fragments of shell, when the examination has been made early by the finger in the manner described. Sometimes, when the finger in the wound fails to find a lodged projectile, the particular spot in which it is lying may be detected simply by relaxing the muscular tissues, so as to give a loose and pendulous condition to the parts concerned, and then lightly tossing up the flesh at different points from below with the tips of the fingers. A bullet lodged among the soft parts will occasionally make its presence known, under such an action, by the impulse which it communicates to the top of one of the fingers when the parts which have been shaken upwards return to their previous position. Sometimes a gentle kneading pressure in the neighbourhood of the injury, assisted by information derived from the sensation of the patient, will lead to the detection of such a foreign body. Sometimes, as mentioned in the English official history of the Crimean war, when a lodged bullet could not otherwise be discovered, it was found by passing the flat palm of the hand down a limb. Its presence was occasionally detected in this way when the points of the fingers had utterly failed to feel it.

We come now to the cases in which the exploration by the surgeon's finger is altogether impracticable, and in which the use of the probe is attended with so many sources of doubt as to prevent any satisfactory conclusion in respect to the lodgment or absence of foreign bodies from being arrived at. These difficulties sometimes happen with wounds in situations

where they might be least expected to be met with, but generally occur in such as have their terminations at or near some of the solid structures of the body. They are every now and then experienced in recent wounds, especially deep and narrow wounds made by small-bore rifle or pistol shot, but are more particularly so in chronic wounds in which foreign bodies are suspected to be lodged; wounds in which the original bullet tracks have become contracted to narrow sinuses, and in which other changes have taken place rendering the directions of these sinuses tortuous or otherwise intricate. The finger may not be able to penetrate the small and constricted passage of such a wound; and the probe, even if it be enabled to traverse it, and happen to reach a hard substance, may fail to give the desired information as to its nature—whether it is striking against bone or the foreign body which is suspected to be lodged.

It may be readily ascertained by striking or rubbing a leaden bullet out of the body with a silver probe, and comparing the peculiar dull sensation conveyed to the fingers with the sensation experienced when a piece of bone is struck or rubbed, that one cannot under such circumstances be deceived with the eyes shut as to the respective differences between the two substances. But when the bullet or piece of bone is at the bottom of a wound, and the probe comes in contact with the side of the wound, especially if this side happen to be bone; or if the walls of the track be fleshy, and, while the probe is in contact with them, its extremity is pressed against bone which has become smooth and eburnated on the surface; or if any soft tissues intervene between the end of the probe and the object impinged upon it; it will be found such a complicated sensation is given to the fingers that the diagnosis is rendered exceedingly difficult and uncertain.

No more remarkable illustration of the difficulty of diagnosis just adverted to could be adduced, perhaps, than was afforded in the instance of the wound received at Mentana by General Garibaldi. In that case, the opening presented to the surgeon beneath the integument consisted of a fissure across the base of the inner malleolus. This fissure was not wide enough to admit a finger; and ordinary probes, when inserted, so failed to give satisfactory evidence on the important question of a foreign body being impacted in bone near the ankle-joint, that some of the ablest surgeons in Europe, after exploring with them, were led to declare that no bullet or foreign body had become lodged in the wound. And this occurred in a case where the opening made by the projectile was not much more, if any more, than an inch in depth, in which the track was not tortuous, nor among tissues of different kinds or of intricate arrangement, as frequently happens in doubtful cases.

M. Nélaton, after his visit to General Garibaldi, was led to think of various devices for determining whether the ball was lodged in the wound or not. His first idea was to obtain a steel probe cut like a file at one extremity. He presumed that, by passing such an instrument down to the substance which was suspected by some to be the bullet, and by giving it a rotatory motion, sufficient would be brought away on the teeth of the file to determine its nature, whether bone or lead. At the same time that he was having this instrument made, he reverted to the idea of a chemical reagent, which had been tried before in similar cases of doubt, but without success. Acting on this motion, M. Nélaton applied to M. E. Rousseau, the chemist, to furnish him with some ample means of determining the presence of lead in a wound by chemical analysis. M. Rousseau then suggested the introduction of a body capable of bringing away a metallic impression, should metal be present, such as rough porcelain; thus making the metal capable of being recognised not only by chemical reaction, but by all its ordinary characteristic physical signs. This suggestion led to the construction of the instrument which, since its successful application in Garibaldi's case, has become known as Nélaton's probe.

Nélaton's probe consists of a slender rod of metal, five or six inches in length, terminated at one end by a small knob of white, unglazed, biscuit china. The other extremity of the probe is furnished with a small handle, grooved ridge-and-furrow fashion, in order that the finger and thumb may the more easily roll it between them, while the porcelain knob is being pressed at the bottom of the wound against the suspected foreign body. If it be a leaden bullet against which the porcelain is rubbed, a very distinct mark of lead is impressed on the latter, which is not easily obliterated. The bullet itself is thus caused to give ocular demonstration of its presence and place of lodgment. If the foreign body be iron, having a rusty surface, a stain of rust will be found on the china.

The round ball of china which is fitted to the Nélaton test-probes usually has a diameter of rather more than a quarter of an inch. This is often large enough to cause a difficulty in introducing it through the small fistulous tracks among fibrous tissues which are frequently met with as one of the chronic effects of gun-shot wounds. A sinus of this kind, unless connected with necrosed bone, usually leads to suspicion in the mind of the surgeon that it is prevented from becoming completely closed by the lodgment of some foreign body which passed along it at the time of the original wound. It is, therefore, just one of the cases in which such a test-probe offers itself as a valuable diagnostic aid to the surgeon, but where the size

of the round china knob too often prevents the attainment of this object. A probe tipped with a piece of the biscuit china of less diameter, and more oval in form, is required for these narrow fistulous tracks. In a recent wound, the larger round ball is more convenient. It is not so likely to be impeded in its passage along the wound, and the impression made by the lead upon it is more obvious to observation.

The porcelain test-probe was much used during the war of the rebellion in the United States, and many favourable reports of the results of its employment have been published. A very ingenious modification of it, but one of doubtful practical utility, was manufactured during the war by the eminent surgical instrument-makers, Messrs. Tieman, of New York. In this instrument, the porcelain is placed at the extremity of a long flexible gum-elastic tube, which takes the place of the metallic stem in the ordinary probe. The length of the whole instrument is thirteen inches, the flexible part being nine inches long. By turning a screw at the handle in different directions, and by means of a mechanical contrivance concealed within the tube, the lower part of the instrument, at the end of which the porcelain is placed, can be caused to turn slowly round to the right or left, at the pleasure of the operator. It may be moved round so as to assume a considerable curve in either direction.

This instrument has obviously been designed with the purpose of applying the porcelain test in cases where the ordinary Nélaton probe cannot be used; more especially along deep and tortuous bullet tracks, whose ultimate direction is either uncertain or known to be turned from a straight course. The operator is supposed to cause the porcelain to *search* for the foreign body, as well as to identify it when found by bringing to light the usual evidence of its presence. I have only tried the instrument on the dead body, but the experiments have not been attended with favourable results. The chief sources of failure with it, were found to be, firstly, the uncertainty concerning the direction taken by the end of the instrument when turned in search of the bullet among yielding tissues, at distances far removed from the aperture of entrance and from sight; and, secondly, the difficulty of applying sufficient force to get a metallic impression, even when the point of the instrument was directed against the foreign body, owing to the flexibility of the long tube at the end of which the button of china is placed. Another difficulty experienced was the following. If the china knob became entangled in the tissues at the end of a long track, or was prevented from moving freely by any cause, then the screw, not being able to act on the extremity, caused the middle flexible portion of the instru-

ment to become curved, so as to press on one side the soft tissues of the track of the wound, along which the probe had been passed. It was only through the displacement of the soft tissues being visible externally after it had occured to some considerable extent, that a knowledge was gained of the fact that the porcelain end was not moving in obedience to the screw, but only that part of the probe lying in the track of the bullet.

It is evident, from the nature of the porcelain test-probes just described, that direct and firm contact between the porcelain and a bullet is essential, in order that the former may furnish the evidence which the surgeon requires. If it be merely a little blood, serum, or soft coagulum, in front of the bullet, pressure by Nélaton's probe will squeeze it away, and the leaden mark can be obtained; but if any resisting medium, however thin—the thinnest membrane, for example—happen to be placed between the surfaces of the metal and the china, no impression will be made on the latter. And there are various substances which are liable to be interposed—such as muscular or cellular tissue pushed by the knob of porcelain itself before it; pieces of linen, cloth, paper, or other substances which have entered with the bullet; a piece or edge of bone projecting in front of it, and other such matters. The evil in such occurrences may not simply be the impediment to obtaining an impression from the lodged bullet; but possibly a surgeon may be led to an erroneous conclusion that he has obtained proof of no foreign body being lodged, because the usual evidence of its lodgment is absent from the porcelain. This may lead to delay in the healing process and protracted suffering to the patient, which might have been avoided had a more correct diagnosis been arrived at.

To obviate these difficulties and sources of fallacy, a surgeon of the French army, Dr. Lecomte, invented an instrument, to which he gave the name of "probe-nippers" (*stylet-pince*). His design was not merely to indicate the presence of a leaden bullet, by bringing away a stain or mark of its presence, but by bringing away a small portion of the lead itself. Such an instrument could not only be used for bringing away a scale of lead, but also a minute portion of paper, cloth, wood, or any other foreign body capable of being cut in a similar manner. If the supposed foreign body were a fragment of bone, it would equally bring away a particle of it. It was evident that, in addition to its other qualities, the *stylet-pince* would require to have its nippers smaller in size than the porcelain knob of Nélaton's probe, with a stem of sufficient length to be passed along narrow sinuses, and at the same time solid enough to bear a certain strain in use. I have ascertained by practical experience that the *stylet-pince* does possess the qualities aimed at by Dr. Lecomte.

The instrument consists of two portions. The first is a central steel rod of small diameter, fixed in an ivory handle at one extremity, and cleft at the other into two small branches, each of which terminates in a little cup-like blade or curette; the second is a slender canula, which glides backward and forward, but only within a limited distance, along this rod. The central rod is fixed in the handle by means of a side-screw, which can be loosened at pleasure by the surgeon, so as to increase the length of the exposed part of the stem, or to enable it to be removed altogether to be cleaned. The two steel curettes have very fine and sharp edges. They separate from each other by the elasticity of the two little steel branches, of which they are the terminations; but they are easily brought together by a slight pressure, such as that exerted by causing the canula to glide along the central stem up to them. When they are thus brought together, the two curettes so fit one to the other that, united, they form a small smooth steel knob or rounded extremity, about one-third of the usual size of the china knob of a Nélaton probe.

It will be apparent, from the description, that the gliding of the canula determines the opening and closing of the curettes: when it is slipped back, the curettes open; when it is pushed forward, they are closed, and form a little hollow globe. There is no difficulty in the manipulation of the instrument. It is inserted with the curettes closed, and it may be used then precisely as the long silver probe in a surgeon's capital case of instruments. When about to be employed for determining the nature of the substance with which it is put into contact, the canula is drawn towards the surgeon, at the same time that the extremity is retained with an equable and steady pressure against the substance. This movement has opened the curettes. The same even pressure is sustained while the canula is pushed home, and this causes the curettes to be brought together again; their edges, as they close towards each other, nipping off a small particle of the substance over which they are moved. The instrument is then withdrawn, and, supposing it to have been brought into contact with an ordinary bullet, a small scale of lead will be brought away enclosed within the cavity of the rounded extremity formed by the two curettes. The glistening surface of the freshly cut shaving of lead will sufficiently indicate its nature. If any difficulty should occur in distinguishing the particle which has been brought away, it can be removed from the curettes, and observation under an ordinary magnifying lens will show what the substance is. The *stylet-pince* is thus a most useful explorator for deciding doubtful cases of lodgment of foreign bodies; it responds as an indicator with even more distinctness than the Nélaton probe in all cases in

which that test would be of service, while it answers for a variety of other cases in which the Nélaton probe would give no indication at all.

Electricity was some years ago suggested as a means of detecting lodged bullets and other metallic substances in wounds; and some very ingenious apparatus depending on this agent have been lately contrived for the same purpose. One of the first to experiment upon an electric appliance of this kind was a French military surgeon, M. Fontan, assisted by M. Favre of Marseilles. A description of the somewhat complicated apparatus employed in these experiments is to be found in the *Gazette des Hôpitaux* of the 29th November, 1862.

The improvements which have taken place in the modern applications of electricity have paved the way for more simple and yet more sensitive bullet explorers. One of these is the invention of Mr. De Wilde, a civil engineer, and is very compactly arranged in a box of small dimensions. The electric action is excited in a suitable cell; the electricity there developed is increased in intensity by the intervention of a multiplying coil; an exploring probe is connected by insulated wires with the apparatus; and the indication, when the circuit is completed by contact of the two points of the probe with a leaden bullet or piece of iron, is given by the striking of a hammer against an alarum-bell. The bell sounds at each interruption and renewal of contact of the points with metal. The exploring probe consists of a long slender tube of smooth vulcanite, containing two insulated needles, the points of which can be withdrawn within the tube, or be made to protrude, at the pleasure of the operator. Altogether it is an effective appliance as an exploring instrument, owing to the strength of the electric current developed, and the marked manner in which the indications are given by the sound of the bell when a bullet or other metallic substance is met with. There is also attached to the instrument a bullet-extractor, the two arms of which are insulated, and so arranged that, when they are connected, in the same way as the explorer, with the battery, they indicate the grasping of the foreign body similarly by the sound of the bell. Unless the metal be firmly grasped by both blades, without any other substance intervening, the indication will not of course be given.

Another instrument of a similar nature has been made by Messrs. Krohne and Sesemann of London. The indications of contact with a lodged bullet or other metal are in this instrument afforded by the movements of a galvanometer, and of a fine needle working upon a dial-plate, in the same manner as is seen in the ordinary single needle telegraph. I have experimented with both these instruments, and have found them

equally effective in their indications. Attached to the latter instrument is not only a bullet-extractor as well as the explorer, but also a pair of acupuncture needles, for use in cases where metallic bodies are supposed to be lodged in soft tissues, away from any means of approach by a wound or sinus.

A rough but sufficiently effective electric instrument for facilitating the discovery of metallic substances lodged in gun-shot-wounds has been made in the following way. The magnet of an ordinary pocket compass, which has had some turns of wire covered with thread wound round it as an induction coil, is employed for the electric indicator, while a piece of copper sheeting, bent round a small plate of zinc, but separated from it by flannel padding saturated with the usual diluted acid, forms the voltaic pile. The exploring instrument is formed by two insulated wires, bound together, but with the points left free. These parts being connected, when the circuit is completed by contact with metal, the indication is given by movement of the magnet of the compass.

Lastly, the endoscope has been suggested for use in exploring for foreign bodies in wounds. Dr. Fenger of Copenhagen, in 1869, made some experiments with the instrument on horses, and came to the conclusion that pieces of cloth in wounds, or bullets driven into bones, could be seen by its means. Dr. Fenger has stated that during the late war he was enabled in several instances, on examining wounds some weeks after they had been inflicted, to see their interiors distinctly by means of the endoscope, without causing pain, hemorrhage, or any subsequent irritation, in consequence of the introduction of the instrument. I have no experience of such an application of the endoscope, and can hardly think that, with so many other effective appliances available, it is likely to be turned to much practical account in this direction.

It must be sufficiently obvious that several of the exploring instruments just described are not suited for use in the field. The circumstances of gunshot-wounds themselves, as they are ordinarily presented shortly after their infliction, do not render such instruments necessary; nor are some of them—as the electric-explorers for example, which may be easily disarranged—suitable for use in the places where the wounds resulting from battle are usually treated during their earliest stages. But it has not been the purpose of these remarks to discuss either the nature or circumstances of the wounds in which instruments of this kind may be specially serviceable; their object has been only to describe the instruments available for exploring for foreign bodies in cases where the exploration is decided to be advisable. It is only right to remark, however, in conclusion, that even in such cases there must be a limit to search. This

limit must depend on the circumstances of each particular case, and must be decided by the judgment and tact of the responsible surgeon. As a general rule, it may be stated that, whatever may be the reasons for concluding that a bullet or any other foreign body has lodged in or near a gunshot-wound, if, after search by the finger in cases where a digital examination is practicable; after external manipulation and observations made in varied postures of the part of the body concerned ; after attention has been given to indications derived from the patient's sensations; effects of pressure upon, or injury to, nerves; and, lastly, after a moderate but careful exploration by one or other of the exploring instruments just described, if, after these steps have been taken, the site of the lodgment be still not ascertained, more especially if the patient be suffering pain, or is in an exhausted condition, the exploration should be at once discontinued. The continuance of the search will not merely add to the weariness and distress of the patient, but, if the several proceedings above-named have been properly carried into execution, it is not likely to be attended with a successful issue. The foreign body will probably have passed beyond or out of the field of exploration, or have become deeply impacted in bone, or entangled among tissues, where it could only be discovered by an unjustifiable amount of meddlesome and injurious disturbance of the structures implicated. We must rest in hope that, either during the process of suppuration, or under the influence of muscular actions, or by gradual approach toward the surface, the escape of the foreign body, whatever its nature, whether hard or soft, smooth or rough, organic or inorganic, may be eventually effected without such risks ; or, if it be of a favourable kind and form, such as a leaden bullet is when its normal shape is retained, and if it be not in contact with a nerve, bone, or other important organ, that the wound may heal favourably, notwithstanding its presence, the foreign body becoming encysted, and remaining lodged without causing either pain or mischief for many years afterwards.—*British Medical Journal, Dec.* 23 *and* 30, 1871, *pp.* 715, 751.

---

## 50.—ON SKIN GRAFTING.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

[The well-known experiments of John Hunter, more than a century ago, may be said to be the first attempt at grafting on an animal body. The first attempt at skin-grafting was made in 1869 by M. Reverdin, at Paris, and the practice at the present time may be said to be an established one.]

For more than one year I have very extensively carried it out, and in most instances with success. I look upon the suggestion as a very valuable one, its adoption rendering many cases curable that were not so previously, facilitating the cure of as many more, and giving interest to a class of cases that had formerly but little. In the management of ulcers it is a great boon, and in the treatment of the large granulating surfaces so common after extensive burns its value cannot be over-estimated; as an adjuvant to many plastic operations, more particularly on the face, and in the case of deformities, it is invaluable.

Under the action originated by the transplanted skin centres, a process of repair goes on which at first appears almost magical; the grafts soon become islets of skin, round which cicatrization proceeds; the margin of the sore receives an impulse in cicatrization, which rapidly extends; and between the grafts themselves and margin of the sore connecting links of new skin rapidly form, which divide the sore into sections. By these means large surfaces speedily cicatrize that under former circumstances would have required many months, and that, too, without the contractions and subsequent deformities that under other conditions were too well known to follow in such cases.

The practice seems applicable wherever a large granulating surface exists, and the only essential point to observe is that the *surface of the sore should be healthy;* this clinical fact including another, that the patient's health is good, for there is no better barometer of health than a sore, its surface assuming a healthy or unhealthy appearance with every alteration in the general condition of the body. I have attempted, by way of experiment, to graft skin upon sores that were not quite healthy, and have sometimes succeeded; in an indolent sore, in which a small patch of healthy granulations sprang up, I have succeeded in securing, by transplanting, a new centre of "cutification," that proved of great value in aiding the healing process, but in a larger number I have failed in securing such an end. It may, therefore, be accepted as a truth *that a healthy granulating surface is an important requisite for success.*

Upon this basis I will now proceed to consider how the operation is to be performed.

Pollock tells us that Reverdin's method is to remove a very minute portion of the skin, and to replace it on the surface of the granulations, and there retain it with a strip of plaster. "I," he writes, "have usually removed the skin by nipping up a very small portion with a fine pair of forceps, and cutting it off close with sharp scissors. At first I made a slight cut in the surface of the granulations, and then imbedded the piece

of skin; but of late I have only laid it on the surface of the ulcer. I cannot say that I have found any difference in the result. I do not think there is great, if any, advantage to be gained by the transplantation of a large piece, but where the ulcer is large I think much is gained by the transplantation of numerous small pieces. The disadvantage of transplanting a large piece is the sore it creates; while the small sores formed by the removal of the minute pieces heal in a short time, and without trouble." (Clin. Soc. Trans., vol. iv.)

My own experience confirms in every point that of Pollock.

I employ for the removal of the sound skin a pair of scissors which Messrs. Khrone made for me after Macleod's suggestion in a medical journal, and find they answer far better than anything else, for they take away a portion of skin that will cut into three or four pieces, and the section includes only the upper layer of the true skin, with the rete mucosum; they do not cut into the fat beneath the skin, or divide the papillæ sufficiently far to draw blood or give pain. Patients never object, with this instrument, for a second or third piece to be taken away, should it be deemed necessary. I generally take the skin from the forepart of the arm or the side of the thorax.

Having taken away the skin, the fragment should be cut into three, four, or more pieces, and then placed about *half an inch or three quarters of an inch from the margin of the sore, and about one inch apart.* There is no doubt that the engrafted centre has a stronger influence in exciting a healing action in the margin of the sore when placed near it, than when isolated in the centre of a granulating surface away from the margin.

These pieces should be placed upon the granulations, and gently pressed in. There is no necessity to wound the granulating surface. They should be covered with a piece of oiled gutta-percha skin, and the whole supported with cotton wool; a bandage being subsequently applied, so as to press moderately upon the part, and keep the dressing in position. On the third day, but not before, the dressings may be removed, the greatest care being needed; a fresh piece of oiled gutta-percha skin being subsequently applied.

The appearances of the engrafted pieces on the removal of the first dressing vary considerably; at times they will seem palpably to have taken root and to be alive; at others to have disappeared altogether; whilst in a third class the surface of the cuticle will be seen floating, as a thin film, upon the secretion of the sore, the basement membrane of the cuticle—the essential part—being left.

Under all these circumstances, however, the surface of the sore is to be cleaned with the greatest care, a stream of tepid

water, either squeezed from a sponge or injected from a syringe over the part, being the best means to employ. The surface is on no account to be wiped, for the grafted portions of skin are easily uprooted, whilst those that appear to have died or that have disappeared often reappear at a later period as "cutifying centres." As soon as the new centres are established in large sores, other pieces should be engrafted, at about the same distance from the new pieces as these were originally inserted from the margin of the sore; and in this way the whole granulating surface may be speedily covered with new skin, and a rapid recovery take place.

How the engrafted pieces act in the healing process is not yet satisfactorily settled, and Reverdin's questions, as already quoted, have not yet been completely answered. That they act as direct stimulants to the sore itself, and more particularly to the margin of the sore, there can be little doubt, for as soon as the "grafts" have taken, the margin of the sore nearest to them is seen to cicatrize and to send out prolongations of new cicatrizing tissue to meet similar prolongations from the new cutifying centres; the sore in this way becoming subdivided by bands into smaller sores, and then rapidly healing.

That the engrafted portions grow by the proliferation of their own cells is likewise proved by the fact that in the case of a white man upon whose ulcerated leg I engrafted four small pieces of black skin, the whole being no larger than a barley-corn, the black skin grew twentyfold in ten weeks, the black portions gradually enlarging and sending out prolongations, which joined, till one patch of black skin had formed, the ulcer healing as rapidly where the black skin was grafted, as where the white was placed. The engrafted pieces of new skin thus themselves grow by cell development, as well as exciting a new skin-forming power in the granulations near which they were placed, and at the margin of the sore; the process of repair by this operation including far more than Mr. Dobson believed when he asserted "that the act of transplantation is simply to insert a pattern of skin, and this pattern is impressed on the granulations to a certain but limited extent; it simply supplies them with a proper model whereby they may fashion themselves." (Med. Times. Oct. 29, 1870.)

That the new pieces of inserted skin may do this is highly probable, but that they do what has been already described is undoubtedly true. They grow themselves, they stimulate the skin-forming powers of the margin of the sore, and exert at the same time a like action on the granulations around.

The same result may likewise take place when large pieces of skin are transplanted, whether removed from the patient or from the amputated limb of a second; the dressers at Guy's

have frequently engrafted large pieces of skin, half an inch square, removed from amputated limbs, the pieces on removal being dropped into warm water in their passage to the wards where they are applied. But there seems to be no advantage in adopting this practice in the majority of cases, a large proportion of cases failing. In one case the grafts took root and excited a healthy action in the margins of the sore; they, however, grew but little; they remained, on the cicatrization of the sore, as bosses of skin with well-marked borders; they were grafted, it was true, but the grafts seemed to have no power of assimilating themselves with the tissues on which they were placed.

To take large pieces of skin from the patient's own body is an objectionable practice, on account of the large wound it creates; it is, moreover, unnecessary in the majority of cases, as small pieces appear to do better. To take them from another subject is also objectionable for like reasons; but still more so on account of the difficulty that sometimes accrues from a want of power in the new graft to assimilate itself to the tissues on which it is placed, and from the risk that is necessarily run of introducing into the blood of the living subject some new or poisonous element; a risk that I believe it to be wrong and unjustifiable to run, and that I would not allow on my own person. On that account I have forbidden my dressers adopting the practice.

In the case where black skin was transplanted I did it with the full concurrence of both patients; indeed, both were rather disappointed that the operation could not be repeated. They were firm friends, and the link I formed bound them closer.

There seems, however, no objection to mincing the portion of integument which is to be employed into minute fragments, that is, into pieces the size of millet-seeds; the thumb-nail of the surgeon being the best table for the purpose; in children, where it is unadvisable to remove much healthy skin, and the surface to be covered is large, the plan is a good one; I prefer, however, pieces the size of half a hemp-seed, when they can be obtained. Recently, the practice of applying "skin dust," or the products obtained by scraping the skin, has found favour, and met with some success. In the few cases in which I have tried it I cannot say I have been successful; the ulcers healed, it was true, but I have no evidence to show that any new cutifying centres were established by the proceeding. In the hands of Mr. Poland it has, however, been of value. I believe the idea originated with M. Marc Sée, of Paris, who called it "epidermic grafting." I might add, as an objection to the practice, that the scraping of healthy skin is far from painless, and to take dead cuticle is useless.

The clinical points alluded to in this paper are well seen in the drawings annexed. They have been carefully and ably taken by Mr. Clarke, my reporter, from the living patient, and from some excellent models lately made by our well-known artist, Mr. Towne, from patients under my care in the wards of Guy's Hospital.

They pretend, however, to show only the most striking features in this process of repair, the endless variations in the process requiring the close inspection of many cases.

Whether this newly engrafted skin possesses the same power of resisting disintegrating changes as the old skin is not yet proved. Some observations I have made lead me to suspect that it is somewhat prone to break down and ulcerate, on the patient walking, after the sore has completely healed, particularly when the subject is past middle age; they are enough to show the necessity of observing as much care in the after-treatment of the case as ought always to be observed after the cure of any other sore; that moderate support and protection are most valuable; and for this purpose there is nothing better than the binding on of a piece of sheet lead over the cicatrix when the seat of mischief is on the leg, as by it equal pressure is supplied to the seat of the original sore, as well as protection.

The new skin soon becomes as sensitive as the old; indeed, the sensibility of the cicatrix, under these circumstances, seems to be greater than it is when unaided cicatrization is allowed to take place.—*Guy's Hospital Reports*, 1872, *p*. 237.

---

## 51.—CASE OF SPINA BIFIDA TREATED ANTISEPTICALLY.

By Dr. JOHN WILSON, Physician to the Glasgow University Lying-in-Hospital.

[The infant was a fortnight old, and the tumour the size of half a billiard ball, with the usual semi-transparent gelatinous aspect.]

We resolved, while keeping in view the risks incurred by all modes of treatment of such tumours hitherto reported, to make a free incision, under an antiseptic veil, and to dress it subsequently with great care, antiseptically. By the free incision, instead of small punctures which might soon close, we hoped to prevent the possibility of the cavity being even partially distended in future, while at the same time by the antiseptic, to obviate all danger of inflammatory disturbance of the cord or its membranes; the ultimate object being to secure complete obliteration of the cavity and the formation of a firm pad over the void in the vertebral column. The plan was duly carried out, and with gratifying results. By the end of the

first month from date of operation, a flattened cake of the condensed tissues had formed, which has remained firm during the four months since then. The details of the case are these.

1871, May 22nd. A. I., aged two weeks. A well-formed, healthy boy. Spina bifida tumour, about size of half a billiard ball, in region of upper dorsal vertebræ, soft, semitransparent, covered by thin membrane. Integument surrounding base of tumour extends up on its sides a short distance, then abruptly gives place to thin membrane. Fluid contents begin to ooze on very slight handling through two or three superficial punctures made by midwife. Infant evidently suffers pain whenever tumour is touched. Walls are felt to be thicker than they appear to the eye. They probably consist of many tissues more or less altered or incomplete, *e.g.*, arachnoid, dura mater, muscular fasciculi, fasciæ and integument.

25th. Tumour first moistened over with carbolized oil (1 in 8), and then opened by free longitudinal incision (about three-quarters of an inch in length) under an antiseptic veil of surgeon's lint soaked in carbolized oil. Copious flow of serous fluid (cerebro-spinal) slightly tinged with blood, probably from the incision. Simultaneously with the removal of the lint, a large piece of carbolized lac plaster was applied, and over this another, kept in position by adhesive plaster, one edge being left comparatively free for escape of fluid. A soft folded handkerchief laid over this completed the dressing.

26th. Child has been restless, partly covered with a rash, especially over nates. Has a sickly appearance. Anterior fontanelle depressed. Does not take to the breast as heartily as formerly. Has been vomiting considerably. Urine pale violet colour. Simultaneously removed lac and applied veil, pressure over which caused some escape of fluid. Veil then supplanted by fresh lac and folded handkerchief as before.

27th. Same mode of dressing, Tumour decidedly more solid —little or no fluid could be squeezed out. Child vomiting but not so frequently. Rash gone, except on nates. Urine still cloudy.

28th. Dressed as before. Tumour contracting. Some slight fibrous-looking threads seen extending from incision—exposure even for a moment to examine them, altogether unsafe. Some vomiting, with diarrhœa; slight depression and sleeplessness. Three grains of chloral in syrup every eight hours, and an occasional teaspoonful of toddy.

31st. Child much improved. Vomiting entirely ceased. Rests quietly. Wound dressed as before. On the lac was found the fibrous-looking material in the incision at last dressing. It seemed to be some loose areolar tissue.

June 2nd. Dressings have remained in good position. Very

little discharge, but incision still patent. Tumour not much further lessened. Restlessness, and some more vomiting and purging. Repeat chloral, and give chalk mixture. Three layers of lac applied, to remain for three days.

5th. On removal of lac, slight sour odour could be noticed on it, but no other evidence of decomposition. Dressing re-applied as before. Child still vomiting occasionally and purging frequently. Ordered tincture of catechu, with the chalk mixture.

7th. Vomiting and purging both lessened. No odour on lac.

9th. Purging much less frequent, and vomiting only occasional. Tumour much diminished. At last two dressings child has not cried during process of moving, changing clothes, plaster, &c. Its mother also says it is less sensitive to motion.

12th. Tumour continues about same. Diarrhœa better, but still continues. Ordered gallic acid with brandy.

15th. Diarrhœa much better.

18th. Dressed with antiseptic gauze, six plies, in place of lac. Child doing well.

21st. Gauze changed. Not a trace of discharge.

(About this time several hard nodules were discovered in the larger muscles of the arms and legs, and seeming to involve portions of these muscles themselves. They were painful to the touch and when the limbs were moved. The mother gently stroked them several times a day with oil, and they very gradually disappeared.)

26th. Gauze changed and tumour inspected, as there was still no trace of discharge. It was found much contracted and flattened and incision healed. Child lively and well.

July 24th. Since last date the mother of the child has managed the treatment, keeping a small piece of the gauze over surface of tumour, in case there should be any oozing. Tumour contracting. Still a small patch of thin membranaceous covering in centre, *but no fluid in sac.* Child well and thriving.

Sept. 15th. Summoned to see child in consequence of an accident. A young girl had been allowed to carry the infant out of doors for a little, when they were knocked down on the pavement by a large dog, and the infant fell out of her arms on its head. Found the whole head much swollen; anterior fontanelle distended; face very pale, as well as general surface of body; disinclination to remain for any time at the breast; distressed crying and at times a degree of torpor. Back, however, had apparently received no injury. Ordered evaporating lotion to head, and sp. æth. nit. in cold water, at intervals.

(The child gradually recovered from this shock; and the head seems now, 19th Oct., to have regained its natural proportions.)

30th. Sac obliterated, and external tumour diminished considerably since last note.

October 19th. Child thriving, and very lively. Strong in back and limbs. Flattened disc, representing tumour still contracting. Small area in centre, about size of half a sixpenny piece, still covered by thin membrane. Gap in vertebral arch is felt to be nearly, if not quite closed, as if by cartilage. Child about to be vaccinated, being nearly six months old.

It is well-known that spina bifida tumours when left to themselves, almost invariably take some such course as the following,—they gradually increase in size, and their walls get thinner and thinner; if they be not ruptured accidentally—which often happens—superficial ulceration takes place, which ends in an opening by which the cerebro-spinal fluid escapes more or less rapidly; air is admitted into the sac, and even to the cord itself; inflammatory and suppurative processes set in, which speedily prove fatal. A few cases are on record where the fatal issue has been averted by treatment for a longer or shorter period; but the methods employed seem in every instance to be fraught with danger. Sir Astley Cooper kept a lumbar tumour constantly down with a truss, which was worn by the boy at school, where he "runs, jumps, and plays about as other children;" but when the truss was removed, the tumour "soon becomes the size of a small orange." Another, lumbar tumour Sir Astley treated by successive punctures with a needle and constant pressure, so that it underwent adhesive inflammation, and became contracted and firm (Med. Chir. Trans., Vol. ii., p. 323, 326). The patient in that case was 28 years of age when we last hear of him. Mr. W. Martin Coates, of Salisbury, a few years ago, treated with success a lumbar spina bifida tumour in the following way,—chloroform being administered, he withdrew by a Wood's syringe a small quantity of the serous fluid, replacing it by an equal quantity of a solution of iodine and iodide of potassium. The tumour thereafter gradually soldified. At the time he reports the case the child "runs about as vigorously as other children of her age." Last year, Dr. Lloyd Roberts, of Manchester, brought before the Obstetric Society of London two cases treated by passing setons through base of tumour. With regard to the first of these—"In a few hours the child became convulsed, and died on the evening of the next day." The second child was alive ($2\frac{1}{2}$ months old) at date of report; but pus was continuing to be discharged through the small openings by the side of the liga-

ture (Obs. Trans., Vol. xii). Keeping these several methods in view, I have already sufficiently indicated our reasons for preferring the antiseptic method with free incision, and in the meantime, at least, the condition of the infant is satisfactory enough to warrant a similar procedure in like circumstances. In addition, it might be advisable to give chloroform, so as to lessen as much as possible the nervous shock undoubtedly caused by the incision and the sudden flow of cerebro-spinal fluid.—*Glasgow Medical Journal, Nov.* 1871, *p.* 1.

---

## 52.—THE USE OF SETONS IN THE TREATMENT OF STRUMOUS DISEASES.

By Dr. Edward Crossman, Hambrook.

Few writers on strumous diseases fail to mention setons or issues among the various remedial measures; but mention is made of them in a cursory manner, and without that stress being laid upon their employment which, in my experience, it deserves. Moreover, they are generally described as applicable only to the early stage of these diseases, while in my opinion they are equally serviceable in the climax.

The wife of a farmer—one of an eminently strumous family —had during two years consulted various medical men, and, immediately before consulting me, had received her death-warrant from a leading physician. Her left lung contained a large cavity; the right was universally crepitant; and she had had several attacks of hæmoptysis. I was not consulted with any hope of cure; but, as she had come home to die, I was called in to watch her end. In this case, from the time when I put a seton in her chest, she began to mend. One set of silks having cut their way out, I put in another; and for six months a continuous discharge was kept up from the skin, with so happy a result that, fifteen months later, I attended her in a confinement, and she has lived six years since.

A young woman, aged 19, presented herself to me, with extensive tubercular disease of both lungs, which had been running a constantly progressive course for more than two years. She was extremely emaciated, had profuse perspirations, and her legs were œdematous. Her case was one of advanced tubercular bronchitis. I put a seton of six silks in her chest, and at the end of a fortnight the symptoms began to abate. She wore the seton three months, by which time her cough had nearly ceased, and her expectoration was *nil*. She then pressed me to remove the seton, and, upon inquiring the reason of her urgency, she informed me that her banns had been twice asked, and she intended to be married the following

week. She *was* married to a worthless fellow, who used her very badly; and, after an interval of eighteen months, I was called on to attend her in an attack of acute tubercular pneumonia, under which she soon sank.

I will not weary you with the details of cases. My memoranda supply me with the records of thirty-five cases of confirmed phthisis in various stages, in thirty-two of which marked benefit was derived from the employment of setons, while in three the seton was removed without benefit. In none of these cases had I reason to regret the introduction of the seton: emaciation and debility were not increased by the discharge from the skin, but, on the contrary, their progress was arrested *pari passu* with the diminished discharge from the lungs.

Passing to another variety of strumous disease—*strumous ophthalmia*—an issue or seton in the temple is the most effectual remedy, and often the only means of stopping the progress of the disease. Probably in the present day setons and issues are used more often in the treatment of strumous ophthalmia than in that of any other disease; and I take it that it is so because their remedial power can, in the eye, be actually seen and appreciated.

It is not only in the true strumous ophthalmia, attended with corneal ulceration, that setons are of service—they are equally so in all those minor affections of the eye and its appendages which take their origin in a strumous diathesis.

For the relief of an eye affection, the seton is usually placed in the temple; but a serious drawback to its use in this situation is the scar which it leaves. I find that, placed in the arm or any convenient part of the body, it has an equally good effect.

Some ten years ago, a girl was under my treatment for many months in consequence of frequently repeated attacks of strumous corneitis. Having failed to eradicate the constitutional tendency by medicines, I put a seton in each temple. The result was a complete cessation of the disease; but, having to earn her bread as a nursemaid, she was naturally anxious to get rid of a disfigurement which prevented her from taking a situation, and, contrary to my advice, removed the setons at the end of a month. Not many weeks afterwards, exposure to cold wind brought back the disease, which again assumed a remittent character. Being in a situation at a distance, it was only when compelled to return home that she again became my patient. I then introduced a seton of six silks into her arm, and kept it discharging the whole winter, during which time she was entirely free from the eye affection, and has never had a serious return since.

A young gentleman seldom passed a month or six weeks for several years without an attack of strumous conjunctivitis. Being first at a private school, then at Eton, and latterly in London, he had been subjected to various plans of treatment. The attacks yielded sometimes to leeching, sometimes to blistering, and at other times to sedative applications; but exposure to cold wind or damp air always induced a recurrence. At last I persuaded his friends to allow me to put a seton in his arm. The suppuration was so severe at first that I had great difficulty in inducing him to retain it; he did so, however, and by degrees the fungoid granulations which had sprung up round the points of insertion disappeared, and a healthy suppuration was established. From the time the seton was inserted the ophthalmic attacks ceased; during the three months he wore it they did not once recur; and for three years since he has had but the most trifling threatenings of his old malady.

Two years ago, a younger brother of the same family was threatened with the same affection. I immediately put a seton of one silk in his temple and stopped the disease, and he has had no recurrence.

A third form of strumous disease, in which I have used setons with great advantage, is tubercular meningitis. My attention was first directed to the beneficial effect of a discharging surface in this disease by a case which occurred to me, in company with the late Dr. Symonds, soon after I commenced practice. A child three years of age, apparently in the last stage of hydrocephalus, was accidentally scalded on the chest, and a large suppurating surface was the result. Dr. Symonds was fully persuaded that the case was hopeless, and was equally convinced, when the child recovered, that the extensive suppuration turned the balance in its favour. Since then I have treated a considerable number of cases of this disease by the introduction of a seton in the poll, and am fully persuaded that, when the attack is sufficiently prolonged, the establishment of a free discharge offers the best hope of cure.

There is yet another form of struma in which I have used setons with advantage—viz., chronic glandular enlargements. I have found obstinate enlargement of the axillary glands yield to a seton in the arm, or below the clavicle. A seton above the clavicle or in the arm will facilitate the removal of enlargement of the cervical glands; and not long since a most obstinate enlargement of the inguinal glands, which for many months resisted all treatment, eventually yielded to a large seton in the arm, kept discharging for eight weeks. Lastly, I may mention chronic strumous ulcers as curable, when other means fail, by a discharge established at a distant part of the body.

I have avoided extending this paper by the narration of cases, and have omitted the mention of any concomitant treatment. I would not have it supposed on this account that I advocate the use of setons to the exclusion of other remedies. I apprehend the part the seton plays is that of evacuant—distilling, as it were, from the blood the morbid material. It remains to check the reproduction of this material by catalytic remedies.—*British Medical Journal, Dec.* 16, 1871, *p.* 693.

---

## ORGANS OF CIRCULATION.

---

### 53.—NOTES OF CLINICAL LECTURE ON TORSION, AND ON THE DRESSING OF WOUNDS.

By George W. Callender, Esq., Surgeon to St. Bartholomew's Hospital.

[In seven important amputations which occurred in Mr. Callender's wards, during three weeks in January, all the arteries (three of which were the femoral) were closed by torsion.]

There is, of course, no absolute novelty in the use of torsion, for quite recently Mr. Cooper Forster has placed before the Clinical Society evidence in its favour; but it is strange that this method of arresting bleeding from a divided vessel should have received so little attention since its first introduction by Amussat. After some trial had been made of it, it quickly fell into disuse, chiefly because Dupuytren reported that it was not safe in the case of large arteries; and now, although attention has been recalled to the operation, its employment is a rare exception.

And yet it admirably fulfils the purpose for which it is practised. Thus the very idea of torsion comes from observation of the way in which Nature herself closes a torn artery. The practice of it insures the laceration of two coats of the vessel and the formation of a coagulum, whilst (in this respect unlike the ligature) no foreign body is left attached to the artery, to be separated from it eventually by ulceration through the outer tunic. The twist of the artery, the tearing of the inner and middle coats, and the formation of a clot—these three — provide for the permanent occlusion of the vessel. There is no record, where the operation has been properly practised, of any sloughing of the twisted end, or of any abscess along the track of the vessel; and, whilst the presence of a foreign body in the wound is avoided, the patient escapes the anxiety which the prospect of the removal of ligatures entails. And to add one other, and this a strong argument, in favour of torsion, it is free from all risk of that secondary bleeding which is sometimes associated with the separation of a ligature.

When employed some forty years ago, various methods were used. By some the end of the vessel was twisted off; by others it was advised to hold the vessel transversely a short distance from its cut extremity with one pair of forceps, whilst the free end was twisted with a second pair. I recommend you to try neither of these two. By twisting off the end you lose that fraction of security which results from leaving the unbroken twist of the external coat beyond the line of the rent in the inner; whilst by fixing the vessel transversely with forceps you needlessly bruise its tissues—that is, if the vessel be held firmly enough to enable you to twist against the resistance. A third method, originally known as "torsion libre," is that which you have often seen employed; it consists in seizing the end of the vessel with any blunt artery-forceps—those which close with a spring being most convenient in practice; then, with the artery as the centre, the handle-end of the forceps is made to describe as wide a circle as possible, slowly twisting the vessel in its rounds. For a small artery the circle described may be small and the twisting rapid; but for an artery of large size the success of the operation is insured by the slow rotation of which I speak. The artery alone—free, that is to say, from surrounding structures—must be held by the forceps; and then, as the twisting progresses, you should look for a diminution in the resistance offered by the vessel.

At first, supposing the femoral to be the subject of the operation, on relaxing your hold of the forceps they will be turned quickly round by the untwisting of the artery, but after four or five turns of the handle you will notice that this tendency to untwist is much lessened; and I may here add that it is well not to carry the twisting to the point of entirely overcoming the recoil of the vessel. You will then direct your assistant to sponge the end of the artery, so that, on loosening the forceps, you may be able to recognise the smallest escape of blood. If there be none, torsion is completed; if there be slight oozing, it will be necessary to reapply the forceps and to give the vessel a further twist. According to the condition of the arterial coats the amount of torsion necessarily varies, and in atheromatous vessels the coats often give way very quickly; but in the case of an old man with atheromatous arteries, on whom amputation at the fore-arm was performed, Mr. Morrant Baker informs me that he had no difficulty in securing the vessels by carefully applied torsion.

When you look at a femoral artery which has been twisted, you will be surprised to see how small is the portion of the vessel included in the knot-like twist—not above one quarter an inch of its length has been disturbed. There is no considerable separation from the sheath or from surrounding structures;

and from the number of wounds which heal by the first intention after torsion, there is every reason to believe that the life of the twisted extremity is not spoiled. Sometimes the appearance of the twist is modified by a little swelling, as if from blood which had escaped between the tunics of the artery; but I never saw this assume any but the smallest proportions, and its occurrence is of no consequence.

I have already told you that after torsion there is no risk from secondary bleeding. If the artery have not been properly secured, it is, however, possible (although I am not aware of any instances of its having happened since the revival of torsion) that it may untwist before the formation of a clot and adhesion between the divided coats have permanently secured it. Such untwisting must occur within the first few hours after an operation. Having myself no experience or fear of such a recurrent, as distinguished from secondary bleeding, I mention the possibility of its happening only to impress upon you the need of care in looking to the proper securing of the vessels at the time of the operation.

Speaking generally, I should say that wounds have healed far oftener by the first intention since I have used torsion than when other modes of arresting hemorrhage have been employed. All the amputations were healed in this way, or with a suppuration so small as to be referable to some corner only of the wound. So, too, with wounds from lesser operations, of which I have in mind a large and deep wound which resulted from the removal of a cancerous tumour from the wall of the abdomen, and several wounds from the removal of cancerous mammary glands.

The advantage of torsion was recently shown in an operation for the repair of a torn perinæum, when two considerable vessels were twisted, after bleeding of so troublesome a character as would otherwise have necessitated the use of ligatures, which I should have been reluctant to employ because they must have been left to traverse and perhaps irritate a wound which it was most important should be united by the first intention.

There are on the other hand, some circumstances under which it is almost impossible to use torsion. In the case of an excision of the knee-joint, some of you may have noticed that I could not twist the arteries which bled from the fibrous tissue on either side of the articulation. The vessels were held so closely in this tissue that either they could not be seized by the forceps, or, if seized, could not be turned round amidst their dense surroundings, so they had to be tied.

When I refer to the better healing of wounds since torsion has been practised, I touch upon a subject which has at present great interest for us. In addition to the attention which Pro-

fesser Tyndall has attracted to the subject by his observations upon the atmospheric surroundings of wounds, Dr. Burdon Sanderson, in a very instructive paper, has brought to our notice the presence in water, such as our wounds are constantly cleansed with, of bacteria or microzymes, as being a probable cause of putrefactive changes; and certainly it is well that we should be reminded of such possible dangers. It is remarkable how rapidly as a rule wounds are repaired in our wards. When we read or hear of the care taken over the wound, for example, of a compound fracture, we recall the fact that the loss of a patient from compound fracture, after the shock of the accident has been recovered from, is, in our experience, an exceptional occurrence. Thus during the last two years, of fourteen cases of compound fracture which have been treated in Kenton, Harley, and Lawrence wards, by Sir James Paget or by myself, thirteen have recovered — the one fatal case having been managed by the antiseptic method; and I attribute this result, which is an illustration of the general well-doing of wounds in our wards, to the care which has always been taken of their first dressing, and to the avoidance of all but absolutely necessary disturbance of them.

There are some details which have to be considered in the making of wounds if we are to secure their healing by primary union. The incision, for example, should be clean, leaving, so far as is practicable, a smooth even surface. Thus in making a circular division of the muscles of the thigh in an amputation, one sweep of the knife sufficies to divide all structures down to the periosteum. The surface of the wound should be handled tenderly—no rough sponging or washing of it should be permitted. All bleeding must be carefully staunched. In the case of a plastic operation for the cure of a torn perinæum, you will almost certainly fail unless you make a point of thus absolutely stopping all hemorrhage and of removing all clots, before you bring together the surfaces of the wound: and what is so well known of this operation holds good for all others. The time, too, which is occupied in the securing of vessels is not lost, for it permits that glazing over of the wound which indicates the moment most favourable for closing it, and which is the first step towards repair.

In the case of an amputation, the edges of the wound are brought together with silver sutures, inserted close to one another, so as accurately to oppose the cut margins; the stump is then covered with lint, once folded, and previously dipped in carbolic oil of the strength of about 1 in 30. Pads of lint or of cotton-wool are then applied above and below, so as to envelope the wound, and the whole dressing is secured with a few turns of a bandage. When the patient is placed in bed the

stump is supported on pillows in the position which gives him greatest ease, and, if necessary, is steadied by means of a broad bandage passed over it and secured to the bed.

You will notice that I follow a pretty uniform plan in the first dressing of wounds, of which the principles are, first, the leaving of the surfaces to Nature, after carefully arresting bleeding and after placing them in apposition; secondly, thorough covering up of the wound with lint or with cotton-wool. As to carbolic oil, or simple olive oil, which I sometimes employ, it is used as a soft and supple application to the flesh. The late Mr. Stanley was in the habit of using in this way, as a local application (I believe at the suggestion of Mr. Gatty), the balsam of Peru; and whilst Mr. Edlin was house-surgeon in 1862, Sir James Paget began, and afterwards continued, the use of carbolic oil as a cleanly dressing for amputation and other wounds. Intermediately Dr. Stenhouse had recommended to us the use of powdered charcoal; and this was employed between layers of cotton-wool, with the view of keeping wounds sweet and free from putrefactive changes. Whether the balsam or the carbolic oil or the powdered charcoal was in repute, its use was combined with the covering of the wound with lint or with cotton-wool; and it cannot but interest you to observe how well this old-established practice fits in with the recommendation, by Professor Tyndall, of cotton-wool as a means for protecting wounds from the impurities of the air.

Following the progress of the case in the wards, you will observe, the next day, that no change has been made in the dressing, unless, in consequence of the patient's complaint of tightness of the bandage, the house-surgeon has cut through some of its folds. It is always necessary to pay careful attention to such a complaint, as the so doing averts the restlessness which neglect of it is sure to engender. And so, too, on the second or third day no change is required. On the fourth the bandage is cut through on either side, and the entire dressing, which has been kept soft with repeated applications of oil, is slipped off without trouble and without disturbing the stump; and we expect to find that the wound has healed, as it had healed in each of the seven cases of amputation of limbs.

There are cases in which, from a variety of causes, primary union cannot be attained: for instance, after such an operation as one which I performed quite recently for the removal of a recurrent colloid tumour from the inner side of the thigh. A large tract of skin was taken away, and the internal saphenous system of veins was destroyed. The very large wound which resulted was dressed in the manner I have described, and subsequently water-dressing, and occasionally poultices, were had

recourse to. The process of repair was hindered in this case by the œdema which resulted from the hurt to the venous system; and it was some weeks before the external saphenous and the deep veins compensated for the loss of the internal saphenous vessels.—*British Medical Journal, Jan.* 20, 1872, *p.* 64.

---

## 54.—NOTES OF A CASE OF POPLITEAL ANEURISM TREATED BY COMPRESSION.

By G. E. Walker, Esq., Surgeon to the Liverpool Hospital for Skin Diseases, &c.

[The patient was a man of middle age who stated that about four hours previously his foot slipped in getting out of a railway carriage and he came with his whole weight (14 stone) on the toes, just resting on the station platform. There were the usual signs of a popliteal aneurism, the size of a small egg.]

In forty-eight bours, in spite of the use of tourniquet, digital pressure and flexion, the aneurism had increased in size so as to bulge out considerably beyond the lateral and posterior boundaries of the ham, and to extend 2½ inches above the adductor opening. The rapidity of the enlargement led me to believe that there had been rupture of the outer, as well as of the middle and inner coats of the artery, though if so the aperture must have been at first very small.

On the fourth day the aneurism and the treatment together had produced such distress as to induce me to request Mr Stubbs to see the case with me, and to urge the propriety of ligaturing the superficial femoral. Mr. Stubbs' advice, however, was to continue the pressure. Up to this time we had endeavoured to keep up continuous pressure day and night, but owing to the bad state of health of our patient, superadded to the ordinary difficulties of the process, we had been unable to prevent the increase in size before mentioned. There was also great œdema of the whole limb, and much engorgement of the vessels leading to the popliteal vein. Much of this was, doubtless, due to the compression of the femoral vein, involved necessarily in that of the artery. The walls of the sac had, however, attained some degree of strength, so that after a six hours' rest, rendered absolutely necessary by the distress of the patient, the aneurism had not further increased, and much of the swelling of the limb had disappeared. We could now apply pressure only very partially, and for some weeks this was the plan. At bedtime the leg was bent on the thigh as far as could be comfortably borne. He could never tolerate for more than a few minutes such an angle as would ensure stoppage of pulsation; still, we were able to lessen pulsation slightly on some occasions for

several hours. In the morning he was moved to a low iron bed, which had a framework at each end about $2\frac{1}{2}$ feet high. Between these a half-inch manilla rope was tightly stretched, for the double purpose of supporting, and by its elasticity enabling us easily to vary the power of, the weight employed. The weight was made up in this fashion: a disc of brass, $1\frac{1}{2}$ in. in diameter, had a stout pin 6 in. long screwed into its centre; leaden weights of various convenient shapes were threaded on this pin; to the bottom of the disc was glued an India-rubber hemispherical air-pad, covered with wash leather, and a stout string connected the top of the pin to the rope, the object of the whole being to maintain closure of the artery with the minimum amount of pressure. The weight directed diagonally against the edge of the pubic ramus could be borne with comfort for about four hours, but in practice we found objections to its use which led to its discontinuance. The slightest lateral movement on the part of the patient would throw it off, and such was the delicacy of the balancing that under a minute increase of blood pressure, as in coughing or sneezing, the weight was lifted, and pulsation caused. In the afternoon the patient was changed to a sofa, and had on a Signorini tourniquet, or controlled the pulsation by flexion, or still better by crossing the affected limb over the other and placing a tightly-rolled towel underneath the aneurism. This was the treatment carried out, with a few unimportant variations, for many weeks. A tourniquet, with elastic bands, though made by one of the best surgical mechanicians, proved utterly useless; the old horseshoe, with an air-pad, answered very fairly.

It will be observed that we were trusting to the intermittent pressure treatment so much extolled by the Dublin writers; and although we did not cure the aneurism by it, we lessened its size so considerably that on the 27th of March, when, owing, to an attack of acute rheumatism, we were compelled to refrain from all surgical treatment, the girth of the joint was only one inch more than that of the sound limb, the pulsating tumour being about $2\frac{1}{2}$ in. in diameter. For the next fortnight the rheumatism afforded us sufficient occupation, and no attempt was made to control the aneurism, as, indeed, none could have been tolerated. When we again began to attack it, we found some little increase in size, and having now lost all faith in the three sorts of treatment we had tried, namely, intermittent pressure, incomplete pressure—that is endeavouring to shut off almost, but not quite, the whole current of blood to the sac, which I had attempted by using the delicately-balanced weight—and, lastly, the let-alone treatment to which we had been driven by the rheumatism, I tried to induce my patient to submit to continuous pressure. Such, how-

ever, was his dread of the repetition of the torture which he endured in the first four days of his illness that, though endowed with exceptional powers of endurance, I could not persuade him to make the attempt until the 5th of May, when he consented.

For the first few days no alteration was made in the mode of applying pressure; it was merely made more continuous, but after that the delicately-balanced weight was laid aside, and a block tin weight of 14lbs., shaped like a rifle bullet, was substituted. It was kept in position by means of a rod screwed into its flat end and made to pass through a hole in the apex of an elliptical arch of steel springing from a hollow pad, on which the thigh rested. The hemispherical end was placed diagonally against the pubic ramus; it effectually controlled the artery, and had several pounds to spare for emergencies.

After sixteen days of this treatment I found the aneurism smaller, but no signs of its occlusion; it was soft, and there was a considerable space unfilled by clot. For reasons detailed below, I now, after procuring another weight, differing from the one already in use by being about some 2¼lbs. heavier, and having a more conical apex, so as to sink deeper into the soft parts, proposed to make pressure on one artery alone—the common femoral, alternating between the pubic ramus and just above the bifurcation of the vessel. We had even with improved appliances great difficulty in compressing the vessel; the groin was sore, the glands were enlarged, and there was great pain down the course of the artery. Changes in the weights had to be made frequently; they were rendered more tolerable through being cooled by a freezing mixture, but it required all the untiring energy and patience of Mr. F. Young, who assisted me throughout the case, to keep up pressure at all, and in spite of every possible care, the horse-shoe tourniquet, in order to give a rest from the weights, had to be fixed on the superficial femoral several times—a course which, I believe, involved actual retrogression.

On the night of the 23rd, whilst being changed from a hard bed to a soft one, the patient extended his limb violently, throwing out of gear the tourniquet, which had been temporarily adjusted to permit the removal, and thereby caused the aneurism to pulsate fully as much as it had done three weeks before. All our labour seemed, therefore, to have gone for nothing. Next morning showed no change, and at noon, being sick with disappointment, our patient wished to be left to die in peace. To allow him a little time to recover, I removed weights and everything, and laid the limb in a comfortable position, with a cushion under the aneurism and a sand-bag over the knee to limit the pulsation. He had then

some food and wine, and, without being made aware of it, for he had a great objection to the drug, a grain of morphia. He feel asleep at once, and in half-an-hour the weight was replaced on the groin; in two hours he awoke much refreshed. The heavier weight was placed on the lower spot, and another grain of morphia administered. With the exception of a very few minutes, during which time he was in a sleep, almost comatose in character, I sat by him from one p.m. to eight p.m. with my fingers and thumb embracing the aneurism, so that I was perfectly certain no blood had passed beneath the weights, which were changed with the greatest care. Towards five o'clock I had the satisfaction of feeling the previously flaccid sac gradually fill out and harden, and at eight, when I left, matters appeared so promising that I ventured to prophesy, could pressure be kept up for another seven hours, the cure would be complete. At nine another dose of morphia—half a grain—was given, and again the patient slept. Towards midnight the nurse in changing the weights, thought the pulsation had stopped, and soon afterwards, on Mr. Young coming on watch, he established the fact conclusively. As matter of precaution, the pressure was kept up during the night; the patient, now that his six months' anxiety was relieved, bearing the physical discomfort with comparative ease.

The considerations which led me to alter the method of pressure were these. The Hunterian operation, as a rule, causes consolidation of the aneurism in a few hours; if then we could make compression act in the same way as ligation, we might expect similar good results. I therefore followed up the steps likely to take place after the application of a ligature to the superficial femoral, as being the artery in which I had an immediate interest, reasoning thus, the blood would pass in increased volume down the profunda and through its anastomoses with the superior articular arteries and the muscular branches of the popliteal, with the inferior articular, and with the anastomotica magna. By means of some of these vessels, according to the position of the aneurism, the blood would reach the diseased part of the vessel either from above or below, but, whatever the direction, the current would be at first a mere trickle, drop by drop, and as the coagulum already in the cavity is rough, there would be present the most favourable conditions for the sealing up of the vessel, namely, a current not strong enough to make its way through the aneurism, and, opposed to such current, a rough surface highly provocative of coagulation.

Now to make compression act as ligation, it seemed that one had only to make it continuous. But as I had been doing this as well as possible for more than a fortnight without success, I

felt there must be a condition unfulfilled of which I was yet unaware. As I had reasoned out the effect of the ligation, so I did of the compression method which I had used. Granting that my pressure for the time being was perfect, this would be the result. After, say four hours' (an extreme) closure of the artery at the groin, I should expect the blood about to seek a new channel by means of the anastomoses of the gluteal and circumflex iliac arteries above, with the various branches of the profunda below, and therefore, to reach the aneurism through the profunda—its ordinary current being reversed—and the superficial femoral. But before this new channel could be made available,—for it takes some time to make a collateral circulation, as I will show further on,—the pressure would be shifted from the groin to the mid-thigh, where the superficial femoral would be compressed, and the onus of forming a fresh channel transferred from the capillaries of the branches of the internal iliac, and the ascending offsets of the profunda, to the descending branches of the last-named vessel and its anastomoses, with the anastomotica magna and the muscular and articular branches of the popliteal. Now, supposing the change from one artery to another to be made too quickly to allow of the production of an efficient collateral channel for either artery, I was driven to the conclusion—always supposing the pressure was complete—that it was possible to starve an aneurism, and therefore to increase its cavity. For I believe a process of absorption, similar to that which takes place after the consolidation of an aneurismal sac, is constantly going on during what may be styled the life of an aneurism. The clot is a foreign body, which nature is always engaged in removing. When, however, the blood has free access to the sac, the deposition of new material takes place with greater rapidity than the removal of the old, and, therefore, the aneurism increases in size.

This explained what had puzzled me before, namely, the stationary condition of the aneurism during the months of intermittent pressure, and the fact that, after a fortnight's continuous pressure, though the aneurism was smaller it was softer, and a very short time of free access to it of the blood sufficed to render pulsation as vigorous as it was weeks before.

The next deduction was obvious. As I could not keep the weight on one spot long enough to produce the desired effect, I must, in changing it, so choose my second site as to keep up the formation of the one collateral circulation which I had begun. I therefore used pressure only on that small portion of vessel bounded above by the inner margin of the pubic ramus, and below by the origin of the profounda. Treatment according in intention with this reasoning was begun effectively three days before the final act. I believe some of this time was wasted,

firstly, because many times the weight was raised to see the condition of the aneurism, for it was impossible at once to convert patient and assistants to the necessity of obedience to the requirements of the theory; and secondly, to the unfortunate accident by which two days' very hard labour was almost completely spoiled. For the latter, I was responsible, as it was done whilst I was removing him from one bed into another with the well-intentioned idea of giving him a good night's sleep. Unhappily, I caused him so much pain that he extended the limb violently, and started the pulsation in all its old vigour. Doubtless by this time the new circulation was almost finished, for the filling and hardening of the sac began next day, very soon after pressure was again efficiently used.

Such then are the considerations which led me to adopt the treatment by pressure on one artery.

For the more easy carrying out of the pressure treatment, I would submit the following recommendations, which refer specially to the lower limb, but which may be applied elsewhere.

The edge of the pubic ramus is the place where the crural artery is most easily compressed. In the case above related, as well as in others, I have found, by catching the artery with the finger diagonally against the edge of the pubis, it could be closed with less pain to the patient, and whilst the surgeon could keep up pressure in this fashion for considerable time, he could only do so perpendicularly to the bone for two or three minutes. The difference could be well estimated with the weight; it required for perpendicular pressure some two or three pounds more than for diagonal.

The groin ought to be well shaven and powdered very thickly; prepared fuller's earth is the best powder.

But of far more importance is the cooling of the weight. For another case, I would have the upper part of it deeply hollowed for the reception of a frigorific mixture. It was only by keeping the weights icy cold that we were able in the case above related to use them at all in the final act, for crops of pustules were appearing with most annoying rapidity, and the glands were enlarged and painful.

Finally, as there is under the use of complete pressure a reasonable prospect of quick cure, I would enjoin the free exhibition of opium or cannabis indica to dull sensibility, not necessarily to the extent of insensibility, but to prevent worry and fidgeting. Mental anxiety intensifies pain wonderfully. The patient whose case is the text of these remarks, after his mind was set at rest by the knowledge that his disease was cured, bore without a murmur for hours on a sore skin the weight which twelve hours previously he could scarce tolerate

twenty minutes, and this too without opium.—*Liverpool Medical and Surgical Reports, Oct.* 1871, *p.* 66.

---

## 55.—A DOUBLE TOURNIQUET, WITH SHIFTING PRESSURE FOR THE TREATMENT OF POPLITEAL ANEURISM BY COMPRESSION.

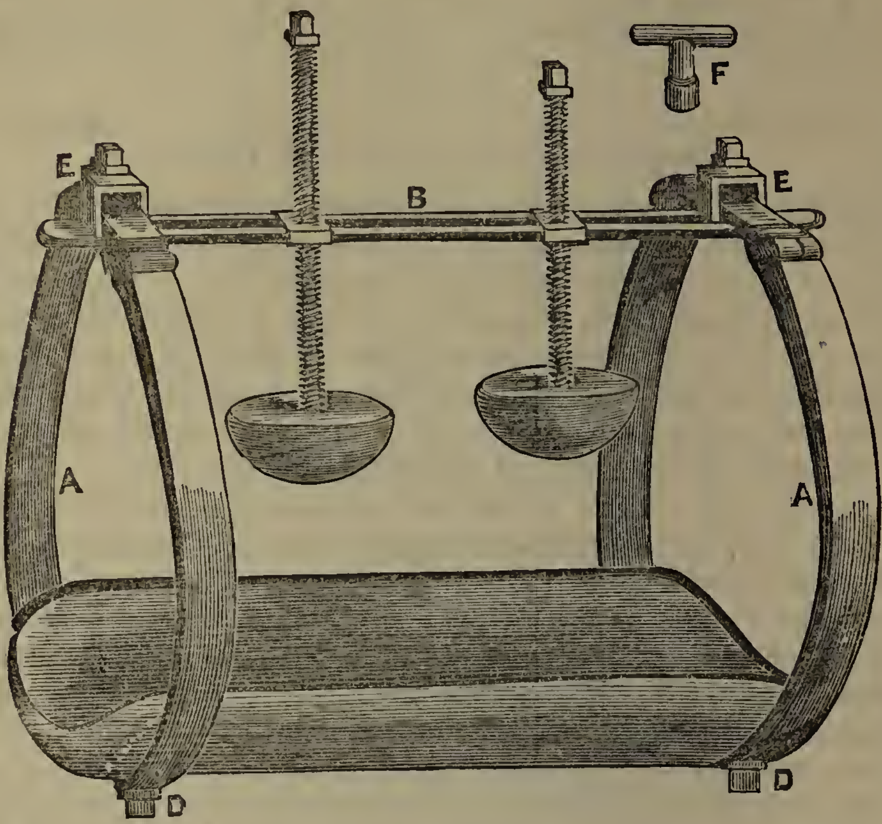
By FRANCIS A. BULLEY, Esq., Consulting Surgeon to the Royal Berkshire Hospital, Reading.

Having on several occasions noticed the difficulty of accurately regulating the pressure upon the femoral artery in the treatment of popliteal aneurism by compression, I was induced to direct my attention to the action of the instruments commonly in use for the purpose, with a view of securing a more exact application of the required pressure, and at the same time insuring as much as possible the comfort of the patient during the occasionally protracted progress of the cure.

It may have been observed, during the treatment by this method, that the single tourniquets usually employed, with the exception, perhaps, of the ingenious invention of Dr. Carte, cannot be made to maintain a fixed and immovable position upon the thigh, without screwing down the compressing pad upon the artery so tightly as completely to obliterate its tube, and in some cases to produce agonising pain in the subjacent nerves, with great venous congestion and swelling of the limb below the point of pressure, thus frequently necessitating the removal of the instrument from the limb. I believe it is now generally admitted that the complete obstruction of the arterial stream is not only not required in the treatment of aneurism by pressure, but, on the contrary, is calculated to defeat the object for which it is designed. If, thus, no blood were allowed to pass through the sac, there could be no deposition and lamination of fibrine in its cavity; and it is only by the production of a languid, retarded stream through the conducting artery that the necessary deposit can be certainly secured. The instrument of which I have given a drawing is capable of producing a regular retardation of the arterial current, with comfort to the patient; and I may further observe that it is in accordance with Dr. Bellingham's recommendation upon the subject. "Compression by two or more instruments, one of which is alternately relaxed, is much more effectual than any single instrument, and in many cases the pressure can be maintained by the patient himself." (Dr. Bellingham on the Treatment of Aneurism, Corollary 5.)

The instrument is composed of two hinged steel hoops, A, united at the bottom by a padded iron plate, and above by a

slotted bar, B, made movable at each end upon the horizontal portion of the hoop, to allow of its adaptation to either limb of the patient. The compressing pads are made to slide easily




along the slot in the bar by means of screw-nuts. The rings can be opened for use or removal by a means of a nut and screw at D, where, also, they may be made larger or smaller, as the size of the limb may require. The whole of the nuts and screws are regulated by a key, as at F. The instrument from which the drawing was taken was made by Messrs. Weiss and Co., of the Strand.—*Medical Times and Gazette, Jan.* 27, 1872, *p.* 95.

---

ALIMENTARY CANAL.

---

## 56.—ON STRANGULATED HERNIA.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

I now go on to speak of the guidance to be derived from the remoter symptoms; and, first, from the inaction of the bowels. This inaction, or rather this hindrance of expulsion, is a nearly constant sign in strangulated hernia, but occurs in so many

other cases that its sole presence is of little weight among motives for operating. Its absence is, rather, that which needs study.

One or more actions of the bowels, after other signs of strangulation have set in, are of no weight at all against the propriety of operating. They commonly occur, because usually after strangulation the part of the bowel below the constricted part empties itself. These actions of the bowels are not to be counted on either side of the question about reducibility or operation; and even a regular and frequent action is not an absolute prohibition, for the strangulation may involve only omentum, or only a part of the circumference of a portion of intestine. In these conditions, fæces may pass along the canal and be discharged.

I had to see a lady for what was considered an abscess in her groin. The swelling was just over the femoral ring, and contained fluid and air. I punctured it, and let out pus and air and liquid fæces, and presently I found a piece of hernial sac sloughed off and lying in the abscess. This, and the history of the case, proved that the hernia had, about a week previously, descended when the patient felt a sudden pain while driving. The hernia was so small that it was overlooked; its pain and the griping which it caused were thought due to colic, and were so treated. The bowels acted sufficiently, and gradually the little hernial sac and its enclosed piece of the intestinal wall died and sloughed off. Then came the signs of abscess over the femoral ring, and its outlet; and this was followed by complete healing and many years of health.

However, such cases as this are very rare; and you may hold by the general rule, that you should not operate when the bowels act frequently or regularly, unless all the other signs of strangulation, both local and remote, be well marked.

The state of the abdomen is as little or less decisive in cases in which you may be in doubt. Of course, if it be sensitive and tender on pressure, either everywhere or in parts near the hernia, still more if it be distended and the muscles hard, this is an addition to the reasons for operating, and, I may add, to the reasons for fearing that you may be too late. It is more to be remembered that, when the abdomen is not tense or tender, when it may even feel nearly natural, yet you must operate if other signs of strangulation be present, and the hernia cannot be put back. For the changes of the abdomen are not common near the beginning of strangulation, and to wait for them would often be to wait till too late.

If I were asked which of the signs of strangulation I would most rely on as commanding the operation, I should certainly say the vomiting. Time after time, when the other signs were

feebly, if at all, marked, the vomiting has been a sufficient guide to a timely operation. Many times, when all else was so quiet that it seemed rash to operate, the vomiting proved that it would have been much more rash to wait; and not one instance can I find in my notes in which neglect of the import of vomiting was not proved to be unwise. You had better hold the rule complete, that, when a patient has a hernia, recently become irreducible, and, with this, vomiting which cannot be clearly assigned to something independent of the hernia, you should operate. I am obliged to say recently become irreducible, because a patient with an old irreducible hernia may vomit, as any one else may, without any reason for suspecting strangulation; though in even these old cases you must be very watchful lest the vomiting be an early sign of strangulation. But the rule is safe that recent irreducibility and vomiting are enough to justify the operation, even though there be no other signs of strangulation present. Much more must the operation be deemed necessary when with these the other signs of strangulation, in even slight degrees, coincide.

And in thus judging of the vomiting, do not be too scrupulous as to its manner or its products. There are indeed some notable modes of vomiting when intestine is strangulated. The patient vomits all he drinks, and that soon after taking it; and besides, he vomits fluids of his own secreting; and this vomiting is commonly (at least in the later stages), with gushes of large quantities of fluid, without much retching or violence, as if the stomach slowly became full of its own secretions and of those of the upper part of the intestine, and then, without any preceding nausea, suddenly emptied itself.

When you see these kinds of vomiting with a hernia lately become irreducible, or even with any swelling that may be a hernia, you may be sure that you must operate. But do not wait for any supposed characteristic mode of vomiting; do not be misguided by the absence of some peculiar fluid; nay, do not be misguided by the absence of all vomiting; for I have known it absent in the case of a very large hernia, which was certainly strangulated, and on which I operated successfully. And do not be misguided by an apparent diminution in the severity or in the frequency of vomiting, or by the vomiting having begun as soon as the hernia descended; for this it often does. Any kind of vomiting, if it be repeated, is enough to justify operation in a hernia recently become irreducible. Let me tell an illustrative case, which taught me the more, because it occurred in one whose life was of great value. He was aged 56, weakly, but laborious in literature. For three days after the descent of a hernia, which could not be reduced as it usually had been, there was not a single sign of strangulation, except

this irreducibility, constipation, and occasional vomiting. He had no pain in or near the hernia; no feeling or aspect of illness; no hardness or tension of the sac; and the vomiting was only occasional, and there were often many hours of interval. But, after the three days, abdominal pain suddenly set in, with coldness and faintness, and wretchedness. Within two hours I operated; but it was too late; peritonitis had already begun, and the operation was useless or worse than useless. He died in twenty-four hours.

Cases like this are frequent. All seems pretty well; and then comes an inrush of indomitable symptoms. The hernia is not acutely inflamed; the patient is not greatly distressed; he flatters himself that he is better, and the similar flattery of his friends is yet stronger: all are averse from operation, and you can hardly persuade yourself to be resolute about it. But there should be no hesitation. I lost the chance of saving this man's life, by underestimating the importance of occasional vomiting as a signal for operation. If you lose such chances, you will be still more blameworthy; for you will have had more warning than ever I had.

And once more; do not be deceived by the cessation of vomiting in the extreme condition of strangulated hernia. This sometimes happens; but it is a token of evil rather than of good, if general improvement do not coincide with the cessation of vomiting. So, again, sickness may be stopped by narcotism; but here again there is no evidence of such general improvement as might justify waiting.

In the recent stages of strangulation, if it be not very acute, the respiration and pulse are little affected. The pulse is usually accelerated, and at first may be full and firm. I find that it was between 80 and 90 in a large majority of the ordinary cases which I have recorded; and the respirations are, generally, I think, in due proportion to the pulse. As the other signs of strangulation become more marked, these, I think, always coincide with them. The pulse usually becomes quicker, feebler, smaller, unless indeed after the warm bath, when its strength and size may be greatly increased; the respiration, I think, keeps pace with the pulse.

Thus, these signs corroborate the others in urging to an operation. I have not any notes or knowledge of cases in which the pulse or respiration was so inconsistent with other signs of strangulation as either to justify or to forbid the operation. But you may have this for a safe rule; that if, while you are watching a case, doubtful whether there be strangulation, the pulse and breathing should increase in frequency, you may believe that there is a commensurate increase in the reasons for operating.

Lastly, as to the patient's aspect and general condition, little that is definite can be said. You read of an anxious expression, and it is called characteristic, as many other fallacious things are. I should rather call it an expression of distress or of misery; but, however you may name it, be careful not to think that you must see it before judging that it is right to operate for a strangulated hernia. The worse a patient looks or feels; the more he looks shrunken, worn, and old-aged; the more miserable his sensations; the more is it unlikely that his strangulated hernia will be reduced without operation. But the reverse is not true. I have operated with full right, as proved by the result, on patients who neither looked nor felt miserable or anxious in any sense of the word. And I have seen a patient looking well and tranquil, in whom a femoral hernia, after seven days' strangulation, contained completely gangrenous intestine.—*Brit. Med. Jour., April* 13, 1872, *p.* 389.

---

## 57.—ON THE REDUCTION OF UMBILICAL AND OTHER HERNIÆ.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

[The following is from a lecture on umbilical hernia.]

A golden rule for practice I believe to be this: never attempt to reduce a hernia except with the patient under the influence of chloroform. I believe more harm is done by violent taxis when the patient is straining than by letting the bowel alone. Strong language this, but I repeat that so much damage is done to the intestine by those who know nothing of the injury they inflict as to counterbalance much of the good which is unquestionably done when cases are operated upon early. You had much better give an anæsthetic, and so do away with all chance of muscular effort, than push and squeeze, and push again, for one, three, ay, four hours, or several times during the evening—a history of magnificent effort which has not unfrequently been narrated to me. Make, then, one decided attempt under chloroform; and if it fail, operate. You cannot hear this too often, because forcible taxis and delay in operating are the great causes of death of all cases in our hospital. First comes the private medical man—he tries perhaps several times; then the dresser; then the house-surgeon; and then the advanced student who may happen to be looking on; and, lastly, the surgeon is sent for, and he very wisely at once operates. I cannot put the case more forcibly than this—that, if I myself had a rupture, no one should try to reduce it except the operator, with the proviso that if he failed after a very moderate attempt he should at once proceed with the knife. I look upon the

operation itself as one of the easiest, the safest, and simplest of the severe procedures of surgery. Cases of umbilical hernia in which operations have been performed are most fatal for this very reason, that so much taxis and delay take place; and, aptly illustrating the converse of this, two cases came under my care, not so very long ago, in which the medical gentleman in attendance showed an amount of intelligence which was quite delightful. I saw both these cases within four hours—one within two—of the descent of the hernia, and in both I operated within an hour of seeing them, and they both got well—a very unusual thing in cases of umbilical rupture.—*Lancet*, *Feb.* 3, 1872, *p.* 143.

---

## 58.—A PLAN FOR FACILITATING THE REDUCTION OF STRANGULATED HERNIA BY TAXIS.

By Dr. Philip Crampton Smyly, Surgeon to the Meath Hospital.

"The objects to be attained in the treatment of hernia in a state of strangulation, are the release of the protruded parts from stricture, and their replacement within the abdomen, provided they are in a suitable condition." These objects are usually sought to be accomplished either by taxis or by operation with the knife."

Some years ago, a nurse in one of the medical wards in the Meath Hospital had a reducible femoral hernia. She neglected to wear a truss, and one day it consequently became strangulated. My father, being the surgeon on duty, tried taxis, as did also the other surgeons, without success. After consultation, an operation was decided on, but every argument failed to persuade the patient to submit—she would rather die than be cut. After the surgeons had left, the clinical clerk (since a very distinguished medical officer in the army) and I thought it a good opportunity to study the relation of the ring to the sac. The result of our examination not a little surprised us. On withdrawing my finger from the ring into which I had inserted it, we heard a distinct gurgle. My fellow-student pressed the tumour, and it passed into the abdomen. The patient lived for many years afterwards, and performed her duties in the hospital. I have since frequently tried to repeat this happy manœuvre, and with most satisfactory results.

For inguinal hernia in the male, the index finger is applied to the lowest part of the scrotum. This is invaginated (as in Wützer's operation for radical cure), the finger being passed behind the testicle and cord up to the external ring. The hernial tumour is then pressed downwards over the finger towards the back of the hand, so as to make the structures in

the ring tense, and consequently smaller. The invaginating finger is then forced firmly upwards and outwards in the direction of the internal ring. As soon as the finger is firmly grasped, the hand should be slightly turned, and the finger pushed towards the middle line. Considerable force may be safely applied in this way, as all the delicate structures are behind the finger, which acts mainly on the stricture. On withdrawing the finger, the hernia can usually be easily returned. The same principle is equally applicable to femoral hernia. This plan may have occurred to others; but if so, it is perhaps not generally known, and any suggestion by which a cutting operation may be safely avoided is acceptable to the practical surgeon. My colleague, Mr. Porter (surgeon to the Queen in Ireland), was much pleased with the success of this plan in a case of inguinal hernia strangulated four days; and he has since tried it himself with satisfactory results.

The advantages which I claim for this procedure are—1. The strangulating portion of the ring is dilated before any pressure is applied to the bowel; 2. Much greater force may be applied to dilate than could safely be brought to bear when the intestine itself is employed for dilatation, as in ordinary taxis; 3. There is much greater probability of returning the bowel into the abdomen in a good condition, and, consequently, in a number of cases avoiding a dangerous surgical operation.—*British Medical Journal, Dec.* 23, 1871, *p.* 724.

---

## 59.—THE TAXIS IN HERNIA.

By F. LE GROS CLARKE, Esq., Surgeon to St. Thomas' Hospital.

In the British Medical Journal of February 10th, 1872, there is a short article from the pen of Mr. Fifield of Boston, U.S.A., entitled, "A Plan for Facilitating the Reduction of Strangulated Hernia by Taxis." This paper contains a long quotation from Baron Seutin on the same subject; and the practice of the Baron is approved by Mr. Fifield.

The proceeding recommended and adopted, I presume, when the ordinary taxis has failed, and therefore when the strangulation is firm, consists in forcing the finger into the hernial ring, whether inguinal or crural, in order to rupture, "by continuous forced dilatation, some of the (strangulating) fibres," whereby a cracking is produced, "very sensible to the finger, and sometimes to the ear." It is added, that "considerable strength has sometimes to be exerted, and the index finger becomes fatigued." In this event "the finger is not to be withdrawn; but it is to be supported by the fingers of an intelligent assistant, who seconds the action it is desirable to produce."

I have never tried this experiment. I should be sorry to do so; and venture to protest against it as a proceeding fraught with danger, and altogether opposed to sound practice, based on an acquaintance with the pathology of hernia. A long hospital experience has taught me how much mischief may be inflicted by persistent violence in attempts to seduce strangulated rupture; and I apprehend that I am not singular in entertaining the opinion that the fatality of our cases is chiefly due to delay, and the repetition of unsuccessful efforts at reduction by taxis.

It is difficult to conceive of any method by which a perilous condition of strangulated intestine could be more effectually aggravated, even if reduction succeed the protracted violence, than by the proceeding here described; for it is idle to suppose that the intestine can be protected whilst this splitting pressure is practised. As compared with a carefully conducted operation, I think there should be no hesitation in giving a preference to the latter.

As an advocate for early operation, and of opening the sac in all but exceptional cases, I strongly condemn the (as I think) fallacious principle of *reduction at all hazards*. I am satisfied that the risks attending an operation for strangulated rupture are very much exaggerated; indeed, as I have pointed out elsewhere, experience has satisfied me that the fatality in these cases is not due to the operation, but to the hopeless condition of the contents of the sac, too often the consequence of delay, or to the prostration of the patient, prior to the relief which an operation is designed to afford.

If gentleness and forbearance were more generally adopted in applying the preliminary measures undertaken to afford relief, and purgatives and procrastination—often self-inflicted—were eliminated from the treatment, I believe that the statistics of the operation for strangulated hernia would present a far more favourable aspect than is at present the case.—*British Medical Journal, April* 13, 1872, *p*. 393.

---

## 60.—ON THE TREATMENT OF ACUTE INTESTINAL STRANGULATION BY PUNCTURE OF THE DISTENDED BOWEL.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

I do not think the Surgeon should any more hesitate in performing an abdominal section than he would hesitate in opening a hernia when symptoms of strangulation existed. I cannot help speaking strongly on this point. I do think the two cases are precisely similar. We all know that in a case of strangulated hernia, if relief is not given, and given speedily,

death is the inevitable result; and we as well know that in cases of acute intestinal obstruction, if relief is not afforded, death must as inevitably occur. Why, then, should we resort to a remedy that proves so successful in the one set of cases, and hesitate to apply it in the other? The only thing that has hindered the Surgeon is a general horror of the operation, and the greater magnitude of it. When you get it associated with the presence of a hernia, we have already seen your course is clear before you. You are bound to explore that; and, finding nothing there, are bound to go up further. But I am talking now of cases where you find nothing of the kind—such cases, I mean, as bands or twists; cases of internal strangulation. Let us consider what means the surgeon has at his disposal for their treatment. If you just ask yourselves what a strangulated bowel really means, you will easily see that other means than those which we have already discussed might be thought of, by which relief could possibly be produced. I will lay before you the line of thought in the way in which it presented itself to me. Here is a piece of intestine passing through a tight ring. We will assume for the moment that the ring is a solid ring—one that does not stretch or expand. The intestine beyond the ring becomes distended and enlarged from flatus or fæces. The bowel becomes strangulated in its unnatural position by a want of fair relationship between the intestine and the ring. It may be an effusion of serum takes place on account of pressure on the vessels preventing the return of blood; the bowel becomes irreducible and inflamed—in fact, strangulated—and if left alone would subsequently slough, just as my finger would slough if I tied a piece of string tightly round it. The bowel becomes strangulated by causes acting within the ring in the bowel itself. Now, I think it is not difficult to understand that if we could empty that intestine of its contents the arrest of the flow of blood out of the intestine would cease, the circulation would go on, the strangulation would cease. Now, you see what I am aiming at as regards the possibility of taking away the cause of strangulation. We cannot take away the constriction of the ring, of the neck of the sac without division; but cannot we give relief through the intestine itself? Let us consider this point.

You know the question of the desirability of puncturing the intestine when distended with flatus has been well ventilated of late. Many cases have been recorded in which the operation has been performed with more or less success. I honestly say that my prejudices have been much against it, although there is every reason why it should have been otherwise, because at this institution it has been tried more than at any other. Mr. Stocker, who has more clinical experience than almost any man

in London, has always been in favour of it. He always used to say, when a man was suffering from wind in this way, "Tap him; the cow-keeper taps his cows and they recover, and why should we hesitate to tap in similar cases?" I have done it myself on two occasions in cases of ileus, and with immense relief. Of course the great argument against it is this: you tap an intestine that is full of flatus, fluid, or fæces, and in putting your needle in there is the danger that some would escape into the abdominal cavity and set up peritonitis. It is solely that fear that has prevented my doing it except in extreme cases. But I think I can give you a case which will help you to form some idea as to the correctness of these conclusions. It as follows:—

*Strangulated Scrotal Hernia (Cæcal)—Herniotomy—Puncture of Intestine—Recovery.*—On August 18, 1871, my friend Mr. Kelson Wright, of Kennington, asked me to see with him a case of strangulated hernia in an old, half-childish man, aged 71. He had been the subject of a right scrotal hernia for thirty years, and had worn a truss. He had had occasional difficulty in its reduction after its descent; but Mr. Wright had always succeeded in reducing it. On the present occasion the same effort had failed, and when I saw him vomiting had existed for two days, and a large hernia existed in the right side of the scrotum; one portion of it felt tenser than the other. Chloroform was given, and the taxis employed, but without success; consequently herniotomy was performed, it being necessary to expose the bowel. When this was done the cæcum escaped, dragging down with it some three inches of small intestine covered with peritoneum—the external ring pressing firmly upon it. With some difficulty the bowel was returned, the wound brought together, and the whole carefully bound together by means of a pad and spica bandage of strapping. A morphia suppository was given.

During the night, however, this old man would get out of bed, and in the attempt he tore off all the dressings. As a consequence, the bowel came down again; vomiting returned, with abdominal pain. Mr. Wright was sent for, but all his efforts to return the intestine were fruitless. I was consequently sent for. I found the old man lower than when I saw him before. The hernia was down larger than ever. I gave him chloroform, and attempted reduction, but failed. I then increased the opening at the internal ring; but on doing this more large intestine came down, and no effort of mine could reduce it. I consequently punctured the intestine in four or five places with a grooved needle, and let off the wind; this measure enabling me to do what, under other circumstances, I could not do—reduce the hernia. The wound was then re-adjusted,

and a good pad firmly secured on with strapping, opium being given; and I am pleased to add, no one bad symptom followed these rough measures, and a good recovery ensued.

Mr. Wright tells me the wound united without a drop of pus appearing, the whole uniting by primary union.

It was interesting to note that when the bowel was punctured nothing but wind escaped, except in one spot, where the smallest drop of blood oozed out, evidently from the congested intestinal walls. None of the contents of the intestines escaped even after the rough manipulation to which they were subjected.

Now that case, gentlemen, I think is a text on which we might go on freely dilating on the benefit of puncturing the large intestine. I think it has given us a great lesson. There was a large piece of intestine entirely exposed, the ascending colon and, I believe, the greater part of the transverse colon distended tremendously and black, not from effusion of blood, but from congestion. I had been pressing and squeezing that intestine in my endeavour to get it back for I should not like to say how long, but it must have been for a great many minutes. It had been, as the medical man expressed it "literally mauled," and we both felt that the old man would die on the table there and then; but I could not stand by without giving him the chance any further interference might afford. So I punctured the intestine, and, after a great escape of wind, I was able to return a part of it. In the second puncture, just a little drop (about the size of a large pin's head) of black stuff oozed out, clearly blood coming from the congested walls of the intestine. I punctured four or five times until I had reduced the whole of it. No fæces escaped whatever, although during the whole of this time I was so manipulating the intestine that if it was possible anything could come out it would have done so. After the patient recovered I asked myself the question: If the large intestine, exposed to view as that was, and not supported in any way by the abdominal walls and contents, could be tapped without any escape taking place, surely, where the intestine is supported, the risk of extravasation would be greatly diminished by the natural support given to it in the abdominal cavity? I think we must draw this conclusion, then: that there are cases in which you may puncture the intestine freely, and with every prospect of affording great relief. In hernia this case clearly proves that you may resort to it, and I believe you had better adopt it if there be much trouble in returning a very large hernia. Had I learned this lesson earlier, I should have tapped long before I did. But might we not employ this treatment much earlier? If, after operating, we can reduce a strangulated hernia by pricking,

which we could not reduce without, is it not possible that we might reduce some herniæ without any operation at all? Consider a small enterocele. See the intestine bulging beyond the neck of the sac and becoming congested. See how it becomes more strangulated and congested if reduction is not effected. If you tap it you let out all the wind, the whole of the knuckle of intestine collapses, and you get only a little flaccid lump. Under these circumstances, what is there to prevent the bowel being replaced by natural agency? I confess my liking for the idea from a surgical point of view. I think it is scientifically correct, and I see nothing to prevent it having the desired effect. I cannot recommend it practically, because I have had no experience of it; but I intend to test the value of it when a suitable case comes before me. And what would constitute a suitable case? Not those slight cases where you can return the intestine without opening the sac, because we know, as a rule, they do well. But I certainly will try it in those cases of hernia that we not infrequently get in hospital, and sometimes even in private practice—large scrotal herniæ, and large umbilical herniæ, the interference with which is nearly always followed by death. It is quite exceptional for a large inguinal or umbilical herniæ to recover after herniotomy when the sac has been opened. By opening the sac I mean not only just at the neck, but complete exposure and manipulation of the contents. In both such cases, then, I shall most certainly apply this practice. Now let us carry this thought a little further. You remember the case I related of a young lady who had strangulation of intestine, evidently from the mere grip of a coil of intestine by a band running from the loin to the bladder. The band would not yield, the intestine could not get out, and every moment rendered the strangulation more intense. Now, supposing I had tapped the intestine, and let out the wind, the bowel would have collapsed, and the bladder, as it became distended, would have allowed the intestine to escape. Then, further, in cases of twist of the intestine you might get much the same thing. When once twisted and distended, it is difficult to untwist, but if the distension is relieved by tapping, you give the bowel a chance of righting itself. Considering, then, with such examples as we have, that tapping is not attended with much danger, surely we are justified in having resort to it before any more serious operative interference, with every prospect of doing good. Do not, however, go away with the opinion that you should adopt it in all cases. But I think, taking that case I narrated as our text, in large strangulated scrotal hernia, strangulated umbilical hernia, and in all cases of acute internal strangulation due possibly to bands or twists, that we may reasonably look for very good success in its practice.

There is one other point I will just touch upon before we separate to day, bearing upon the question of umbilical hernia. You heard me say how very fatal the treatment of umbilical hernia by operation generally is, that it is quite exceptional for recovery to take place. In all the years I have been at Guy's the only cases in which recovery has taken place have been those in which the contents of the sac have never been exposed. The stricture has been divided—I cannot say outside the sac—but an opening has been made only just sufficiently large to allow a herniotome to pass. I have had three or four such cases, and they have got well. In all the other cases, where more has been done, death has followed. I would go a little further, and say—Do not attempt to return an umbilical hernia, they are almost always chronic, slow in formation, and exist for many years before strangulation takes place. I think in large herniæ I would puncture; but when you must operate, never be tempted to expose the bowel—simply cut down on to the neck of the sac, divide the structure by a herniotome, and leave the rest to nature. You relieve the strangulation by simply dividing the neck, precisely in the same way as I believe you would relieve the strangulation by tapping the bowel.—*Medical Times and Gazette, April* 20, 1872, *p*, 455.

---

## 61.—INTESTINAL WORMS.

By William Date, Esq.

Many are the remedies which have, at one time or other, been recommended for tapeworm. It is only necessary here to refer to two or three of them. *Turpentine* acts with a considerable degree of certainty, but, if given in an effectual dose, is apt to produce disagreeable intoxication and strangury. *Kousso* was at one time a fashionable remedy. It is not easily obtained genuine, and (perhaps for that reason) is uncertain in its action and not to be relied upon. *Kameela*, a powder which consists of minute glands from the capsules of Rottlera tinctoria, has been highly lauded. It is said to be certain and speedy in its effects, and not more unpleasant in action than an ordinary purge. I have tried it in a few cases, and found, when given in effective doses, that patients complained of the severe griping pain which it induced. Dr. Garrod says he has found its active purgative properties rather objectionable. For the last few years I have been accustomed to rely entirely on the etherial tincture of the root or rhizome of the male shield-fern (*Aspidium filix mas*); and, when properly administered—*i.e.*, after due preparation of the patient,—I have found it thoroughly efficient. Sometimes, however, it requires a con-

siderable amount of persuasion to induce the patient to submit to the necessary restrictions, which are certainly by no means pleasant nor always convenient. No food must be taken after breakfast except mutton-broth or tea. In the evening the patient takes a brisk dose of castor oil or compound senna mixture. The object of this is to clear away accumulated mucus, and expose the worm to the full action of the vermifuge. The following morning, early, one drachm and a half of liquid extract of male fern, rubbed up with half an ounce of mucilage, should be taken in about two ounces of milk. It is better to lie quiet for two or three hours after taking this dose, otherwise it is apt to cause nausea and faintness. At the expiration of that time another aperient dose should, if necessary, be administered, since the male fern itself does not always purge. Milk is the favourite food of the worm, and is therefore the best vehicle for the administration of the medicine. It is commonly recommended to be taken as food the day before. I regard this as a mistake, as the animal becomes satiated with it, and is thus more likely to avoid the poison. Nothing but broth or tea should be taken until the bowels have been evacuated, unless it be a little brandy-and-water in case of faintness. After undergoing this ordeal the patient may be allowed a week's rest. The whole process should then be repeated. This time perhaps no fragments of the worm will be expelled. I have given the male fern in several cases with perfect success after other remedies had failed. In no instance have I found it necessary to give more than three doses, although I always give one dose after any joints are expelled. Generally the whole of the worm has been killed by the first dose, and thus only two doses have been required. It is seldom the head can be found, owing to its small size, and, perhaps, its maceration in the intestinal juices. The discovery of some of the smaller joints may be regarded as tolerably satisfactory evidence of the destruction of the worm.

Being satisfied as to the entire expulsion of the worm, the next thing is to prevent the development of others. For this purpose, two indications are to be observed:—1. To avoid the introduction of living embryos. 2. To restore the tone of the intestinal mucous membrane, so that it may not offer a favourable nidus for their development. *No meat should be eaten which has not been thoroughly subjected to the action of heat.* It is clear that tapeworms cannot originate *de novo* in the intestine. It may not be possible always to avoid the "measle," but we may always kill it by fire and render it innocuous. To fulfil the second indication nothing is more useful than a mixture of nitro-hydrochloric acid with a bitter infusion. If there be much anæmia, as there often is, a few minims of tincture of

perchloride of iron may be added to each dose, as in the following mixture:—Dilute nitro-hydrochloric acid, three drachms; tincture of perchloride of iron, a drachm and a half; syrup of oranges, half an ounce; infusion of quassia to eight ounces: take one ounce three times daily. At the same time general hygiene should be well attended to. The diet should be plain and nourishing. A daily sponge bath also will be of service. —*Lancet*, *Feb.* 3, 1872, *p.* 146.

---

## 62.—RECTILINEAR ECRASEUR, FOR THE REMOVAL OF HÆMORRHOIDS.

By Dr. J. C. Nott, New York.

[M. Nélaton objects to the use of an écraseur, for the removal of internal hemorrhoids, on the grounds that dangerous hemorrhage has been know to follow its use, and that from the amount of tissue embraced, stricture of the anus not unfrequently results.]

About twelve months ago I contrived a *rectilinear écraseur*, which I think, properly employed, is free from the above objections, and fulfils all the indications better than any operation yet devised. It is almost entirely free from subsequent pain.

This instrument has two parallel blades coming together like a clamp. One blade has a narrow fenestra running its whole length of about three inches, and the other presents a rough edge (like a fine saw) so constructed as to pass into and fill up the fenestra when the clamp is closed. There is a shoulder projecting on each side of the blades, for the purpose of crushing more perfectly the tissues operated upon, a little beyond the thin edge of the blade.

This instrument does not completely sever the tissues (as does the écraseur of Chassaignac), but crushes them down to a very attenuated pulp. If the hemorrhoid is within the sphincter, it is safest to tie a ligature in the sulcus made by the écraseur for fear of some secondary hemorrhage—the tissue is so compressed that a very small pedicle is left for the ligature, and the vitality of the part being completely destroyed the *ligature causes no pain*, and the tumor drops off in a day or two. I cut away with scissors the part of the tumor outside of the ligature at the time of the operation.

I removed five internal hemorrhoids, at one sitting, from a patient, a few days ago, in the presence of Drs. Keyes, Castle, Yale, and Dudley, and after the effect of the anæsthetic passed off, the patient never complained of pain. This is a result which is impossible after ligatures. The clamp and actual

cautery used by some surgeons give good results, but are much more troublesome.

Where the hemorrhoids are external, instead of using a ligature I clip them off with scissors as soon as the clamp is removed.

The principal advantages of this instrument are:—

1st. It is easily and rapidly applied, and requires much less time in its action than that of Chassaignac.

2d. It removes in a *right line* the exact amount of tissue desired, and *is not* followed by pain.

This instrument is applicable to the tongue, cervix uteri, penis, tumors on cervix uteri or vagina, the vagina in operations for procidentia uteri, &c.

I should not omit to mention that after I had worked out in my own brain the above instrument and exhibited it to Prof. Issac E. Taylor, he showed me one he had invented twelve years previously, identical in principle though differing in shape and some minutiæ of construction; his is curvilinear, mine rectilinear, &c. His instrument, though invented twelve years ago, was never published that I am aware of, has not been exhibited for sale by the instrument makers of New York, and having only within the last four years been a resident of this city, it is not remarkable that I never heard of it until it was shown to me by him. His instrument, I think, was designed particularly for the removal of uterine growths; but although the principle is excellent, I think the size and shape make it inconvenient and inapplicable in most cases where an écraseur can be used.

I have also had a pair of artery forceps constructed on the same principle, for arresting hemorrhage in arteries. About both these instruments I shall have more to say on a future occasion. —*New York Medical Record, Oct.* 16, 1871, *p.* 381.

## ORGANS OF URINE AND GENERATION.

### 63.—ON THE DIAGNOSIS AND TREATMENT OF URETHRAL AND VESICAL DISEASES.

By Sir Henry Thompson, Surgeon to University College Hospital.

There are no diseases more painful, and none the relief of which will gain you more gratitude from your patients.

In the matter of diagnosis, however, it is of the greatest importance that it should be a correct one, and not only correct, but rapidly made. I have now to say what I have said to you before, that I interrogate all these patients on the same system, and I advise you to follow this plan. I employ only

four questions for urinary patients, and I advise you to use these four questions also, and always in the same order. The first question is, Is there any deviation in the frequency of passing urine? The second is, Is there any pain in the act? The third, Is there any blood in the urine? And the fourth is, Are the characters of the urine altered (quality and quantity)?

We shall see that in all cases of urinary disease these four questions are sufficient, together with the supplementary inquiries which arise out of them; yet we know how often such cases are misunderstood—indeed, the simplest are often mistaken, through not pursuing a systematic method in arriving at a diagnosis. First of all, let us look at the question of *frequency*. Almost every disease of the urinary organs produces some deviation in the natural frequency of passing urine. As a rule, let it be understood that a man in health does not generally rise at night to pass urine, and that he passes it during the day about five or six times; but when there is any degree of inflammatory action in the mucous membrane of the bladder, however slight, frequency of micturition is induced. Now, how does cystitis produce this increased frequency? When the mucous coat of the bladder is inflamed, it cannot bear to be much extended; and when the bladder contains five or six ounces of urine, or even less, the sensitive mucous membrane suggests that it should be emptied: instead of comfortably containing fifteen or sixteen ounces, it cannot endure the extension, and calls on the muscles to contract without delay. There is one, and one only, of these affections which does not necessarily produce, at first, frequent micturition. I speak of stricture: here it always occurs after a time; but a man may have a considerable amount of stricture for years before he is troubled in the way referred to. Calculous disease produces cystitis, and thus causes an increased frequency in passing urine. Now, as a supplementary question, you should next ask, Is the frequency greatest at night or in the day? If a man have calculus in the bladder, he is not so much disturbed at night, but in the day he is frequently micturating—all movements make him do so. Now, that extremely common complaint, *hypertrophy of the prostate*, is worse at night than by day, as far as frequency of passing urine is concerned. Hence, if a man of about sixty years of age says that he has but recently had urinary troubles, and these are greatest by night, the case is almost made out; you may be sure that a very little further inquiry will demonstrate the fact that he is the subject of hypertrophied prostate.

I come to the second question of *pain*. This question is of greater significance. Suppose the patient says he feels pain. Where do you feel pain—low down in the belly? Then there

is almost certainly chronic cystitis. Suppose he says that his pain is in the penis or perinæum, you must ask if he feels the pain before, during, or after, passing urine. If the pain be before, that is because the mucous membrane is becoming uneasy in consequence of distension. If he find it painful during or after passing urine, and in the end of the penis, he is likely to have stone; and especially, also, if the pain be increased by exercise. The pain is at the end of the penis in stone. It is almost pathognomonic of calculus to find the pain near to the end of the penis during and after micturition. In chronic prostatitis, the pain is also at the end of the penis. This simulates calculus in the bladder more than any other disease.

The third question is, Has *blood* passed? This brings us nearer still to the point. Blood may be seen in cystitis, but very rarely. The mode and the circumstances in which the blood has passed, however, determine the nature of the disease. An elderly man, who passes blood intimately mixed with the urine, dark in colour, and not altered much by circumstances, with frequent rather than painful micturition, has probably hypertrophy of the prostate. In calculus of the bladder you find blood: it is as common in calculus as hæmoptysis is in phthisis. Then a calculous patient will find blood in the urine after a drive or a ride, or after hunting, and none if he keep quiet; or he may pass a drop or two with the last expulsive effort at micturition, and with pain at the time. Such urine is usually rather florid in tint, while, generally speaking, blood passed from the kidney remains long in the bladder, and, from contact with the urine, becomes brown in colour—it is like porter. This, also, may happen when the bleeding is due to hypertrophy of the prostate.

Lastly, Is the *character* of the urine perceptibly changed? A man will often tell you his urine is thick; but he does not discriminate between the thickness of pus or mucus, and that from deposited salts, as lithates. Patients are generally very much disturbed unnecessarily on account of thick urine. In this cold weather, the urine, on cooling, deposits its lithates readily, where none would be seen in summer; and you may tell him that, if he apply a little heat to it, he can see for himself that it will become quite clear again, which is never the case if the thickness be due to organic matters like pus or mucus; and if this be not an habitual appearance, you may make light of it. If, on the other hand, a heavy deposit of lithates be constant, you must look into his habits and correct his digestion—probably restrict some indulgence in diet. If, also, the urine do not become clear with heat, you have an organic compound to deal with, and you must find out carefully the source of it.

Let me advise you always to make your patient pass his urine into two vessels for examination. I should not thank you for an examination of urine passed into one vessel; for whatever a man may happen to have lying in the urethra—a passage which is by no means always clear and sound—passes with it. Let him pass an ounce or two into one vessel, and examine only what you find in the second vessel. If there be gleety discharge, if there be stricture of the urethra, you will find shreds of pus and mucus and blood-corpuscles in the first glass, but not in the second. In chronic prostatitis, always in hypertrophy of the prostate, sometimes there will be a deposit in the first vessel, which would much mislead you if you imagined it to come from the bladder or kidney. This specimen you must examine for albumen, for sugar, and you must inquire also the quantity passed per diem. Well, then, if a patient have told you that he has frequency of passing urine, increased by exercise; that he has pain at the end of the penis; that he passes blood; and that his urine is changed, you may arrive at a pretty good diagnosis of his case. But you would be very much to blame if you did not further examine the man: you must pass an instrument. It is best to be straightforward with patients and tell them so. People have too much common sense to be dealt with otherwise than plainly in these matters. You need not always sound a man with a stiff rigid metallic instrument at first, who has never had an instrument of any kind in his urethra. It is best to take a soft instrument, pass it gently into the bladder, which produces very little discomfort, and so diminish the patient's fear. You can then, say, pass another instrument (which will give you a little more pain), and ascertain completely what is the matter.—*British Medical Journal, Dec.* 9, 1871, *p.* 662.

---

## 64.—ON THE TREATMENT BEST ADAPTED FOR THE PREVENTION OF STONE IN THE BLADDER.

By SIR HENRY THOMPSON, Surgeon-Extraordinary to H.M. the King of the Belgians, &c.

[However perfect the operative means for the removal of stone may have become, there is no one who would not be infinitely well pleased if the formation of stone were prevented, rather than removed, however cleverly you perform the operation.]

Can we do anything to prevent the formation of stone in the urinary passages? It is, in fact, the early stage of this malady that will be the subject of our lecture to-day.

I commence by saying that I think a great deal may be done. But at the outset of the inquiry we naturally ask, What is the

kind of stone (for there are several kinds) the formation of which we may hope to do most in preventing? All calculi are of local or of constitutional origin. By "local" I mean formed by disease in the bladder, and not depending upon any constitutional conditions; by "constitutional" I mean formed by some vice in the system. Now, the large majority of stones are of constitutional, and not of local origin. When they are local you know that we cannot prevent their formation except by mechanical means. Calculous matter, the elements of which are produced in the bladder itself, may be washed out, or may be broken and then washed out. But when stones are of constitutional origin—and those are what we are going to refer to entirely to-day—the elements are separated from the blood, and no mechanical means of prevention can be thought of for an instant. Now, from observation we know that nineteen out of twenty of such stones have uric acid for their basis, the remaining one in twenty being oxalate of lime;* and, less commonly still, there are phosphatic stones which are of constitutional origin also. Therefore, practically, to all intents and purposes, the problem before us is how to prevent the formation of uric-acid calculus.

Let us examine the early history of a case of persisting uric-acid deposit? First of all, let me say, going back to the root of the matter, that it is generally more or less hereditary. As a single illustration of this, we have just seen a case of uric-acid calculus in the ward, and learned that his father had "gravel or stone for the last twenty years of his life." And my almost invariable experience is, that either calculus or gout, most commonly the latter, has been observed in the family of the patient who comes to me with one of these formations in his bladder. I believe it, then, to be very strongly hereditary. We speak of tubercular disease and of cancer as being transmitted by blood relations, especially the former; but I do not think even it is more certainly hereditary than the disposition to uric-acid deposits in one form or another. I make a point of asking the question of all patients who come to me with this complaint; and although I cannot at present furnish you with an exact numerical statement, I do know that in a very large majority, gravel or gout (for I wish to show you the identity as to the origin of the two complaints) has existed in the preceding generation; indeed, it is rare to find it otherwise. This hereditary tendency varies in force or strength in different families. You will find some persons with persisting uric-acid deposits at thirty years of age, others at forty, others at sixty. Of course,

* The deposits of oxalate of lime and of uric acid so often replace each other that the consideration of the latter becomes practically generally sufficient for our purpose.

the earlier the time at which it appears, the stronger you will find the hereditary disposition, and the more obstinate, probably, will be its tendency to persist.

What, then, are the first signs of this condition in the patient? Usually, the first sign is that the urine deposits pinkish matter, on cooling, at the bottom of the vessel, or the secretion may be merely cloudy when cold. It is passed quite clear, becoming cloudy only when it acquires the ordinary surrounding temperature. This phenomenon, therefore, may appear more frequently in winter than in summer, because the external temperature is lower. It is simply a deposit of salts from a hot solution as the liquid grows cooler, all being easily dissolved by raising the temperature of the liquid to that at which it was originally passed. This is a condition of urine which very often and very unnecessarily excites much anxiety on the part of patients, and only the persistence of which can be looked upon as a sign of what is called "the uric-acid diathesis." Mind, I mean strictly persistence, or at all events frequency of occurrence; for you or I, with no hereditary predisposition, may take a little more beer than usual, or an extra glass of champagne, or a glass or two of unaccustomed port, and find next morning a considerable quantity of this pinkish deposit, the urine looking almost like pea-soup, but not so thick; and when the vessel is tilted on one side a tidal mark, so to speak, is seen, showing the height at which the liquid stood; all this, as I said before, being redissolved by heat. But if, without any errors of diet, of which any but a very small allowance of alcoholic drink is only one, a patient habitually passes this kind of urine—if in time there is a frequent deposit of uric acid, manifested by the presence of little crystals, looking like particles of cayenne pepper at the bottom of the vessel —when this occurs rather early in life, say before forty, there is not much doubt that there is a strong tendency to produce uric acid, either inherited or acquired. For this tendency may to a certain extent be acquired, or a pre-existing habit may be intensified; but, as I have before said, it is almost always inherited.

We have followed the complaint up to the formation of cayenne-pepper crystals. I now show you a specimen of urine quite cloudy with lithates, although you must be familiar with it in the wards, and how on heating the liquid it again becomes clear, and that in a short time, while we are talking, it again becomes cloudy on cooling. Let me once more remind you that this may happen with the most healthy individual; and it is only the persistence of the symptom, without errors of diet, which should lead you to suspect a condition that requires treatment. Of the cayenne-pepper deposit I have some very

good specimens here, which have been collected from patients who passed it habitually. These consist mainly of the transparent rhomboidal uric-acid crystals, which you know to be very beautiful objects under the microscope. They may be passed almost daily and habitually, or only every month or six weeks, in large quantities, when they usually produce some irritation. At such periods the patient may experience pains in the back and great discomfort, and be then said to have an attack of sand or gravel. These attacks may occur at varying intervals, and usually become more frequent or severe, unless the patient does something to prevent the formation. Afterwards he passes tiny calculi, popularly called "gravel," which seem to be rounded aggregations of the same crystals; and these little bodies tend in time to become larger, sometimes as large as small peas, or even beans; and they are still specimens of the same product—that is, of uric acid, associated more or less with some base, such as soda or ammonia.

Now let me recall for one moment what I said as to the relation of gout to this condition. I have frequently seen these two complaints alternating, comparing one generation with another: gout appearing in one, gravel in the second, and then gout in the third. But the same individual may also have alternating attacks of gout, and gravel. I have seen a patient suffering for years from gout, which ceased for several months, when he developed for the first time a uric-acid stone. Lastly, the so-called "chalk-stone," which you have often seen infesting the knuckles and disfiguring the hands of elderly people in advanced stages of gout, are composed of the same material—that is, of uric acid, usually as urate of soda. The identity of the two things, then, is unquestionable; they constitute two different series of phenomena, but both spring from the same root.

Now what is to be done for these cases? What mode of treatment will help to prevent the arrival of at least the advanced condition—namely, that of calculus too large to be voided by the patient? Generally speaking, I think we see such patients in a tolerably early stage, although this is by no means always the case. Some are much alarmed at a very early period, when the urine is only occasionally thick with urates. You will of course disabuse such patients of their false impressions, because numbers of persons mistake such thick urine for highly organised matter. I have known persons to become almost hypochondriac through not knowing that such deposits are of little consequence at first, and can be easily treated. But what are we to do for those who habitually pass the cayenne-pepper crystals of uric acid or small calculi? You will first seek the patient's antecedents, and learn all that he has to tell

you of his habits, his diet, and his family history; and your mode of treatment will be determined accordingly. First of all, let me speak of the general principles upon which the treatment should be conducted. A very simple rule—indeed, too simple, I think, is often adopted. When the urine has persistently and habitually thrown down acid deposits, the patient has generally been prescribed alkalies; if, on the contrary, he has had alkaline deposits, he has been treated with acids. That simple mode has too often formed the main portion of the treatment. In the former case he has soda or potash largely administered, or he will be told to drink so many glasses of Vichy water, which is mainly a strong solution of carbonate of soda, only a natural instead of an artificial one. Now it is quite true that with alkalies, provided enough be taken, these deposits will disappear: the uric acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of. And of course the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, "What more can be desired?" This: you have merely made his enemy disappear, but he is by no means rid of its presence; you have not checked the acid formation. The uric acid is there as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, believes itself secure. Just such is the security of the patient with uric acid who trusts solely to alkalies or Vichy water. His surplus deposits have become imperceptible to *his* vision; nothing more. I do not say the alkalies have been absolutely unserviceable as regarding his constitutional state, but they will not improve it to any great extent; and when he leaves them off the acid shows itself again. Diuretics must be regarded in the same light. In those cases which are treated with diuretics, the secretion of water is no doubt increased *quoad* the amount of solids, and the solids are thus dissolved. In both instances what you have chiefly done has been, to stimulate the kidneys, already over-worked, to do more. You have by no means cured the patient.

Now let us ask what is the real pathology of these cases, and then I think I shall be able to show you a more efficient remedy. The problem has presented itself to me with great force and frequency, because people, naturally fearing they may arrive at the stage of calculus formations, come for advice in the earliest stages, and with the strongest desire to avoid the advanced one of stone in the bladder. So far from sending them to Vichy or giving them alkalies, I believe they can be more effectually dealt with by a different mode of

treatment. Let me premise, in broad and simple terms—as our time here, and, I may perhaps add, the extent of our knowledge, will not permit me to be more minute or exact in detail, —that the origin of what we call gouty symptoms, as well as of a superabundant uric-acid deposit in the urine, is due to defective assimilation on the part of organs associated with or forming the primæ viæ. I am quite aware that it is common in practice to speak somewhat knowingly of the liver, its actions and its states, although we have still a good deal to learn about all this. Some years ago we talked and acted as if we were thoroughly acquainted with the liver and its functions; but during the last fifteen or twenty years new light has been thrown upon the subject by Bernard, Pavy, and other workers in the same field, and we have learned that the more we inquired the less did we certainly know of its natural functions, still less of its action in disease. Thus, if one thing was more settled than another, at least since the time of Abernethy, it was that mercury had a specific influence on that organ; but now we find there are grounds for believing that the action supposed has no existence at all. There were other things, indeed, which were vaunted to take the place of it, but no one ever thought of disputing the fact that you could augment the bile secretion by administering mercury. I am not here to say whether that is so or not, but it seems to have been proved that there are substantial reasons for doubting if our ancient faith in that dogma be tenable. In speaking then, of the "defective action of the liver," or "torpor of the liver," I merely use provisional terms, which most understand as indicating more or less distinctly a certain set of symptoms. Let them be briefly described as mainly consisting of a constant, or almost constant, deficient excreting function by the bowels, sometimes, but by no means always, associated with impaired appetite and slow or uneasy digestion; these latter being often absent if the diet is carefully selected, or if the patient lives in the open air and takes much exercise. On the other hand, considerable and multiform symptoms of disturbed digestion may be frequently present. I cannot positively state whether those phenomena are really due to inactivity of the organ in question; practically, for us to-day, this does not signify much, but the current terms are still convenient formularies until others can be substituted for describing the condition in question.

Now, at the bottom of this tendency to uric-acid production there often lies what is thus understood as inactivity of the liver; and the true *rationale* of the undue formation of the urinary salts appears to be that, the liver or some allied organ not doing its duty as an excreting organ, the kidneys have more work thrown upon them. Thus the solid matters of the urine,

or rather some of its ordinary constituents, are augmented,—not all of them, for urea is not necessarily increased, but uric acid is largely produced, and is eliminated not only in solution but in crystalline forms. Uric acid is very insoluble in water, and although the quantity thrown out may be quite soluble at the natural temperature of the urine (100° Fahr.), when this diminishes to 60°, 50°, or 40°, the acid is deposited; and when the quantity is still larger, even the ordinary amount of fluid associated with it at a temperature of 100° will not suffice to dissolve the whole, and solid uric-acid is deposited in some part of the urinary passages. This deposit may take place in the kidneys themselves, giving rise, if not thrown off, to the formation of calculus, at first renal, but sooner or later mostly becoming vesical. Now, if all this be so, the formation of uric-acid gravel is not by any means to be regarded as necessarily disease of the kidney; on the contrary, it is the result of an active and capable organ vicariously relieving some other organ which is torpid. The true remedy therefore is, not to stimulate the kidneys, already over-worked, not, to use a familiar simile, to spur that horse of the team which is already doing too much work, but you are to seek the cause in that other one of the team which is doing deficient work, and that is almost invariably the liver, in the sense already explained.

The treatment, then, which I advise you to pursue is to employ such agents as will stimulate the excretory action by the primæ viæ without depressing vital power. No doubt that a powerful agent for the purpose is mercury; and it is quite unquestionable that the relief of the symptoms above alluded to is to be obtained in a remarkable manner by occasional small doses of that drug. For our purpose, however, it is neither so successful in action, neither can it be considered so harmless, as another class of agents, which in these cases I believe to be in every way superior—I mean certain kinds of natural mineral waters. These I also regard as superior, in these maladies, to taraxacum, nitric acid, alkalies, and the other substitutes, as they have been termed, for mercurial remedies, in prompting the function of the liver. The mineral waters which I refer to belong to a group of springs all containing sulphate of soda, and some of them sulphate of magnesia, also, in solution. In studying these waters, I wish you to look with me at the composition of them, and at the same time to dismiss from your minds entirely those views of midicinal doses which you have acquired in the dispensary, and which necessarily belong to it, since small quantities of drugs, as they exist in mineral waters, will act more freely than will those quantities combined after the ordinary pharmaceutical method. You ask me for a demonstration, and I am quite

ready to give it you. At the same time, let me caution you against regarding the small doses of mineral waters as having any affinity, either in the matter of quantity or by manner of administration, with what is understood as homœopathic doses. Thus, for example, you know that you may give A an ounce of salts, or B half an ounce, and you purge them; but you may obtain the same effect with one-fifth of those quantities if you give it as prepared in nature's laboratory in the form of mineral water. It is a curious fact, which I give as an ultimate one, and without speculating here on the cause of the difference. As a proof of the superior force of the saline combinations found in natural springs, I may refer you to the following experiment. If you will reduce by careful evaporation, as I have done, such mineral waters to their pharmaceutical condition of crystallised salts, you will find them possessing little if any more power than similar salts as obtained by the ordinary processes, and met with in every chemist's shop. They no longer do their work on the same terms as when administered in the original water before they were separated by evaporation. You will therefore readily understand how essential to our end it is to employ the natural mineral waters; since what are called "artificial waters," however admirably prepared, are simply pharmaceutical products, and are destitute of the very quality which distinguishes the remedies they are designed to imitate.

Here is a table of the waters which I refer to, with a comparative synopsis of their distinguishing saline contents, representing the number of grains (without chloride of sodium and other less active agents) in an English pint. Below these I add two well-known alkaline waters.

| | Sulphate of Soda. | Sulphate of Magnesia. | Carbonate of Soda. | |
|---|---|---|---|---|
| *Saline:* | | | | |
| Püllna .. .. .. | 154 grs. .. | 116 grs. .. | — | |
| Friedrishshalle .. | 58 ,, .. | 49 ,, .. | — | |
| Marienbad (Kreuz). | 48 ,, .. | — ,, .. | 9 grs. | Little iron. |
| Carlsbad (Sprudel). | 25 ,, .. | — ,, .. | 13 ,, | |
| Franzensbad .. .. | 30 ,, .. | — ,, .. | 6 ,, | Little iron. |
| *Alkaline:* | | | | |
| Vichy (Celestins) about .. .. | 3 ,, .. | — ,, .. | 47 ,, | Little iron. |
| Vals(Magdeleine) about .. .. | — .. | — .. | 65 ,, | Little iron. |

The most powerful of this group is that of Püllna, which contains 154 grains, or nearly $2\frac{1}{2}$ drachms, of sulphate of soda

to the English pint, and nearly 2 drachms, of sulphate of magnesia. Those quantities would give a tolerably efficient purge to anybody. But you must not give a pint of Püllna; 5 ounces would be a full dose. I do not like Püllna generally for our purpose, because it purges too freely, often gripes, and is very nauseous. Half a drachm of sulphate of soda and half a drachm of sulphate of magnesia in this form is too much for many people. I therefore recommend Friedrichshalle, which contains not a drachm of sulphate of soda in a pint, and little more than three-quarters of a drachm of sulphate of magnesia. Nevertheless, you would not think of giving a pint; eight or nine ounces make an efficient purge; for many persons six or seven suffice. I think I may say that seven ounces is an ordinary average dose; if you take an average dose of Friedrichshalle water an hour before breakfast, and soon after a cup of hot tea or coffee, you will have a full, free action of the bowels; perhaps two. That, you see, would be about twenty-five grains of sulphate of soda, and twenty grains of sulphate of magnesia, which, taken in any combination you like out of a druggist's drawer, would have no appreciable action; you might be a little uncomfortable perhaps, but there would be no action of the bowels. To repeat what I have said: if you evaporate a quantity of Friedrichshalle water in a warm water bath, and obtain as perfect a product as a chemist can produce, and administer four times the amount of salt in that form which exists in a dose of the natural water, you would still not have such efficient or such certain results as from the small quantity (in the natural water) named above. So that there is something, which I do not pretend to explain, and certainly shall not speculate about here, which distinguishes the action of mineral waters from the action of salts which are produced pharmaceutically. The next water on my list is Marienbad, which contains no sulphate of magnesia, forty-eight grains of sulphate of soda in the pint, with nine grains of carbonate of soda, and a small quantity of iron. About half a pint produces for most persons an easy motion. If this water is exposed to the air for a day or two, there will be an obvious brown deposit of the iron, and it may be regarded as slightly ferruginous, though that is a secondary character. The next is Carlsbad with its many springs, all of which contain about twenty grains of sulphate of soda and thirteen of carbonate of soda in the pint. Then we come to Franzensbad, which contains thirty grains of sulphate of soda, six of carbonate, and a little iron, which Carlsbad does not. That closes this group of springs.

But now I shall just point out the distinctive characters of the alkaline waters which are so popular in this country. First,

Vichy, which contains only three grains of sulphate of soda, but nearly fifty grains of carbonate of soda, in the pint—a powerful solution. Then we come to Vals, which is also from the volcanic district of France, some of the springs of which contain upwards of sixty grains of carbonate of soda, and nothing else worth mentioning. These two waters are extremely famous, and are much resorted to against gout and gravel. Under their use the uric acid deposits disappear—that is, they are dissolved by the alkali. Inasmuch also as this appears to have some beneficial action on the liver, a certain degree of permanent benefit is perhaps also attained. Thus such patients are often better for a time after a visit to Vichy; but, as a rule, are not permanently benefited. I am satisfied, after observation on the spot, and on the effect of the waters here, that they only temporarily mitigate the complaint, and do not cure it. Now, the principle upon which the waters of Friedrichshalle and Carlsbad are beneficial is, that they produce activity in all the digestive functions, and thus waste matters which have been hitherto thrown out as uric acid by the kidney are eliminated in some other form. And thus it is that, if it be necessary to send these patients abroad, I prefer very much Carlsbad to Vichy, provided always that the subject of a Carlsbad course must not have become too weak, as for a time it makes a demand on the strength not well supported by a feeble person. Generally, however, this is not at all the condition of those who are passing uric-acid gravel. Happily, also, for most people our purpose can be attained as well at home. I believe that a short course of Friedrichshalle first, followed by Carlsbad, or by a combination of the two, produces the best results in these cases. This method has, at all events, been more successful with me than any other. Such a course should be continued, according to circumstances, for six or eight weeks.

A few words upon the way in which you should give them. If you have a patient coming to you whose digestion is not good, complaining of foul tongue, with deranged digestion and loss of appetite, it is sometimes, not always, desirable to give first a single dose, no more, say three or four grains, of blue pill at night, and the next morning eight or ten ounces of Friedrichshalle water, so as to ensure good action at first. Then commence with Friedrichshalle combined with hot water, one dose every morning an hour before breakfast, diminishing a little the quantity every day, or every few days. One of the characteristics of this water is that the longer it is taken the smaller is the quantity necessary to effect the purpose. If, for example, seven or eight ounces taken in the morning, say with five ounces of hot water, produce one active movement of the bowels immediately after breakfast, the next morning six or seven ounces will

do the same, and the morning after probably five or six ounces; and it is very likely that at the end of three weeks the patient will from four ounces experience the same effect produced originally by seven or eight. But after you have given it thus for one, two, or three weeks, according to the nature of the case and the results, you should combine it with Carlsbad, say three or four ounces of the former and five or six of the latter, with three or four of hot water every morning. When Friedrichshalle is given alone, and also when it is mixed with Carlsbad, 20 or 30 per cent. of hot water should be added, that it may resemble somewhat the original condition at the spring. Friedrichshalle is naturally hot, and is evaporated on the spot to a small extent, being regulated and rendered uniform by stopping the process when the water reaches a certain specific gravity. Carlsbad, which at the spring is too hot to drink until cooled, should, when taken alone and unmixed, be raised in temperature to 90° or 100°, by placing the tumbler, containing it in a vessel of hot water for a few minutes. After giving this combination of the two waters for two or three weeks, six, seven, or eight ounces of Carlsbad may be taken alone for another fortnight perhaps. The quantities given are considerably less than those administered at the spa itself, where a patient's stay is necessarily limited as to time. I am quite satisfied that the smaller quantity here recommended, and employed for six to nine weeks, instead of the usual three weeks of a foreign course, is better for the majority of the patients we have to deal with. The same quantity of water given there in twenty-one days, producing often notable loss of weight and power, will, if given here in fifty or sixty days, attain the object as certainly and more safely. I by no means dispute that there are other cases which may be benefited by the more heroic plan adopted on the spot. I have largely and systematically employed these agents now for seven or eight years, modifying the quantity and the mode as experience has indicated, and the course thus briefly described is the result of it. The course may be repeated with advantage, if necessary, for most patients after an interval of three or four months. Meantime, as an occasional aperient and a corrector of digestion for these patients, I know nothing at all equal to Friedrichshalle. It leaves the patient as a rule less constipated after discontinuing it than he was before; and, as already said, may be taken habitually without lowering the system. I have know patients continuing its daily use for three or four years, but I do not advise this course unless in exceptional cases. Nevertheless I know a gentleman, nearly eighty years old, who has taken it for five years regularly, a wine glass every morning, and the effect has been for him most admirable. Suffering

much formerly from an obstinate constipation, he enjoys perfect regularity and excellent health. Let me say here that what is called "Carlsbad salt" is often used for the same purpose, and is taken also in the belief that it represents Carlsbad water. This it does not do, consisting almost entirely of sulphate of soda taken from the water, and has the same and no more virtue than that salt when obtained from any other source. No doubt that the sulphate of soda, known also as "Glauber's salt," is one of the most admirable medicines we possess, and deserves to be more popular than it is. I constantly order it, with or without a small addition of sulphate of magnesia, for the out-patients as the best substitute within my reach for the mineral waters in question.

In the few minutes which remain I will advert briefly to the subject of diet, certain restrictions in which are extremely important. It used to be said that when uric acid is largely deposited the nitrogenous elements of the food should be considerably diminished. I do not find in practice that a strict application of this rule is advantageous. On the contrary, the diminution of the deposit is more certainly attained by a course which is almost the opposite of that. There are three classes of aliments which must be permitted to the patient very sparingly, in order to attain the end in view—viz., alcohol. saccharine, and fatty matters. First, alcohol: any fermented liquor permitted as an article of diet should be selected in its more diluted and in its most pure form. Believing that form in which it is found existing in natural wines to be usually the best, I would advise a light sound Bordeaux or a Rhine wine of similar quality, the former perhaps agreeing better with most persons here. You will forbid champagne, as for the most part imperfectly constituted, and always bad if containing much liqueur. The stronger wines, as sherry and port, are mostly unsuitable, and strong beer is to be absolutely forbidden. Solutions of pure spirit and water are exceptionally desirable for some few persons with weak digestion. Secondly, sugar in all its forms: at every meal and wherever met with, forbid it altogether. Thirdly, let fatty matters, butter, cream, and the fat of meat, whether simply cooked or in combination to form pastry, be taken very sparingly. I cannot enlarge on the theory on which this advice is given. Suffice it to say that abstinence from the substances named probably lightens considerably the work of the liver, and so lessens the vicarious labour of the kidneys in accordance with the views already propounded. Let me just advert, moreover, to the dietetic system at Carlsbad. In recognition of some such principle there, no doubt, the use of sugar and of butter is absolutely forbidden during a Carlsbad course; and were you patients

there, your purveyor would not supply you with the forbidden food, however much you might demand it. I can only say, as the result of observation, that this system, much more than the elimination of meat from the dietary, will reduce the uric-acid deposit. If you will cut off a portion of alcoholic stimulant when necessary, and it often is so, will forbid everything that contains sugar, and diminish considerably all fatty matter—giving nitrogenous food, in fact, and eliminating hydrocarbons—you will generally accomplish more than by the contrary method. In addition to all this, you will of course see that the patient takes daily a fair amount of exercise in the open air, and that he protects his skin and encourages the performance of its functions by habitual ablutions and sufficient clothing. These points I can only name; but they are essential concomitants to the rest.

Well, then, it is this system of diet and regimen, and the occasional systematic employment of the mineral waters named, which mainly constitute the treatment I strongly advise for the purpose of checking calculous disease in its early stages, and so to prevent the formation of stone in the bladder in that considerable majority of cases which are due to uric-acid formation and its consequences.—*Lancet, Jan.* 13, 1872, *p.* 35.

---

## 65.—REMARKS ON STONE IN THE BLADDER.

By W. F. Teevan, Esq., B.A., Surgeon to the West London Hospital, and to St. Peter's Hospital.

The question of lithotrity *versus* lithotomy in males under sixteen years of age may be considered as settled in favour of the latter operation, for cutting for stone in boys is so very successful an operation that no apparent advantage can be gained by substituting, in them, lithotrity for lithotomy; but the physical development of a patient, rather than his age, ought to determine the question as to which is the earliest period that lithotrity may be admissible in youth. And it must also be borne in mind that the narrow urethra of a boy may preclude the introduction of a lithotrite. The youngest patient in whom I have performed lithotrity was sixteen years of age; but he was large for his age, and his well-developed penis enclosed a capacious urethra.

If possible, lithotrity ought always to be performed in adults in preference to lithotomy; but inasmuch as lithotrity is in reality only applicable to selected cases, it will be seen that in hospital practice a large number of sufferers must always be left for the knife as the only means of cure. As a general rule, it may be said that it is not advisable to crush a stone which is

larger than a walnut. In men who are young and strong a large phosphatic stone may be crushed with safety; but it is not desirable even to crush a moderately-sized calculus in an infirm old man whose health is much broken, or who may be the subject of chronic cystitis, for the increased irritation set up by the operation of lithotrity in a much diseased bladder is likely to prove perilous to life. The state of the bladder in the subject of a contemplated operation has not, I think, received that attention, sometimes, which it deserves. If the viscus be in an irritable state, or be unable to expel its contents without the use of a catheter (not comfortably borne), we shall be exposing the patient to danger if we perform lithotrity; for he will suffer great annoyance either from his ever-recurring desire to micturate or our washing out his bladder, so that his health will probably break down under the prolonged irritation and confinement. If there are several calculi, it is usually better to cut than to crush; and an oxalate-of-lime stone, unless smaller than a walnut, had better be cut out, rather than crushed.

Then, again, there are certain cases—not many, though—in which no instrument shaped liked a lithotrite can be introduced into the bladder, and which must be cut on a very long and curved staff. Thus, therefore, it appears that lithotrity takes out the good cases for itself and leaves the bad ones for lithotomy; the former operation is only applicable in selected cases, the latter embraces all. We may thus at the very outset premise that lithotrity can be performed oftener in private practice than in hospital; for, in the former, patients usually seek advice on the advent of the first symptoms of stone; whilst, in the latter, they very often delay applying to a hospital for relief till forced to do so by the urgency of their sufferings caused by the presence of a calculus of long standing.

Messrs. Mayer and Meltzer have constructed for me, after my own design, a lithotrite which I believe has certain advantages not possessed by any other. It must be a matter of daily observation to every surgeon that a sound or metal bougie glides into the bladder more easily than a hollow metallic catheter; and we shall also find that the heavier the lithotrite the more facile will be its introduction. I consequently have had my lithotrite made with a heavy square steel handle, which form not only permits the greatest facility of introduction, but also unites firmness of grasp when held, with extreme delicacy of manipulation. Then, again, I have substituted a vertical slide for a button, which only requires the pressure of the thumb—infinitely more simple than thrusting a button to and fro.

The great secret of success in passing a lithotrite comfortably to the patient, and without any hitch to the manipulator, is

simply to keep the handle close to the patient's belly, and to delay the turning movement until it is called for by the fact of the lithotrite not going any further, thus announcing that it has arrived at the turning point. The instrument ought to be oiled from end to end, and the penis pulled well forward on to the lithotrite until the beak has passed the subpubic arch. If the patient's prostate be enlarged, a good stiff bolster under the buttocks will make all the difference between success and failure. No instrument glides into the bladder so easily as a lithotrite; it simply requires, not pushing, but directing,—its own weight does away with the necessity of the former action. Any attempt to force a lithotrite into a bladder will be attended with disastrous consequences, especially if the prostate be enlarged. As a rule, the only difficulty in getting hold of a stone occurs when it is small and lying in a cul-de-sac behind a large prostate; and it may be said that the shorter the time that the instrument remains in the bladder the more favourably is the case likely to turn out.

In performing lithotrity, one of two courses is open to us. We can either break up the stone in one or two crushings, or we can subject the patient each week to one very short and partial crushing. If the former precedure be adopted we shall, unless the stone be very small, set up great local and constitutional disturbance and irritation, depriving the patient of rest, and taking away his appetite; if, on the contrary, the latter method be adopted, we shall create but very little annoyance, and the patient will only be confined to his bed the day of his operation, and lose neither rest or appetite; that method therefore must be the better which does not interfere with a patient's rest or appetite. By adhering to the plan of very short crushings I have lately treated several cases with perfect success as out-patients, and believe they made more rapid recoveries than they would if they had been confined to the hospital; it must however, be borne in mind that the only exercise they were subjected to was a short occasional walk at a slow pace.

I object to chloroform being given in lithotrity, for I consider the patient's consciousness a most useful adjuvant to the surgeon in this operation, and I should not allow it to be administered in any case of my own unless some grave reason demanded its administration. I am afraid the act of "washing out the bladder" is looked upon very lightly, but it is infinitely more annoying to the patient than lithotrity, and ought never to be employed unless imperatively needed, for much irritation may be set up by it.

[Some time ago Mr. Teevan instituted a series of experimental researches into the state of parts after operation on the dead subject. The following were the results arrived at.]

1. If the tip of the forefinger be introduced into the prostatic urethra, either from the bladder or through the membranous urethra, it will be found that it will only admit the terminal joint of the forefinger without laceration.

2. If the introduction of the forefinger be continued, the mucous membrane splits horizontally as the second joint is passing through. The urethra splits in the roof because the convexity of the joint is pressed against that part. In lateral lithotomy the incision into the prostate prevents laceration of the roof of the prostatic urethra.

3. If a stone, half an inch in diameter, be extracted through a prostate in which no incision has been made, the mucous membrane of the floor of the urethra is lacerated, and the prostate slightly torn ; the capsule remains perfect, but the orifices of the ejaculatory ducts are often with difficulty recognised.

4. If a calculus, half an inch in diameter, be extracted through a prostate which has been partially incised, as in lateral lithotomy, the capsule and the orifices of the ejaculatory ducts remain perfect.

5. Stones upwards of half an inch in diameter, when extracted by the median operation, lacerate more or less the prostate and its capsule, and obliterate the orifices of the ejaculatory ducts.

6. Calculi of one inch in diameter and upwards, when extracted through a prostate which has only been partially incised, in the lateral operation, lacerate the gland and its capsule completely in a direction downwards and outwards, and obliterate the orifices of the ejaculatory ducts.

7. If a stone be extracted through *an aperture made by cutting, and not by laceration, then the orifices of the ejaculatory ducts can always be distinguished.*

8. When a calculus is extracted from the bladder by means of a limited incision and subsequently so-called dilatation, either in lateral or median lithotomy, there is always more or less eversion of the gland—that is, in such cases the stone has a tendency to enucleate the gland from its capsule in a direction forwards.

Thus, therefore, only a very small stone can be extracted through a partially incised prostate, without completely lacerating the gland and its eapsule.

The specimens of bladders and prostates after lithotomy which I have exhibited show that after an ordinary-sized calculus has been extracted by lateral or median lithotomy the prostate is always found split in two, the halves being held together by a fibrous remnant of the capsule about half an inch broad.

Some persons might possibly object to deductions drawn from experiments on the dead body, and say that the results obtained after death must be very different to what happened after lithotomy on the living; but it must be borne in mind that the mechanical properties of the fasciæ are not altered for some time after death, and therefore experiments made a few hours post mortem afford results similar to those that would have ensued in the living. I have also made a personal examination of all the pathological museums in London, and have obtained the following facts:—

1. Out of the many specimens of bladders and prostates after lithotomy, there is no unequivocal specimen which shows that an ordinary sized stone can be extracted through a medium sized prostate by means of the limited incision and subsequent so-called dilatation without complete rupture of the prostate and its capsule.

2. That in extracting ordinary-sized calculi through the prostate, not only are the gland and capsule completely ruptured, but the rent extends into the bladder, as far usually as the orifice of the left ureter.

3. That where there has been much laceration or bruising of parts, the orifices of the duct are no longer to be distinguished.

4. That there are several bladders and prostates of persons who have lived upwards of ten years after lateral lithotomy, and in each specimen the cicatrix can be seen extending into the bladder nearly to the orifice of the left ureter.

5. That a fistula in the bladder communicating with the rectum is not an uncommon occurrence after lithotomy—the result of bruising or laceration,—and would not appear to be of any moment.

6. That in ordinary lithotomy the prostate is completely split into two, the halves being held together by a remnant of the capsule about half an inch broad.

7. That the most frequent cause of death after lithotomy would seem to be the extensive suppuration set up by the bruising and laceration of parts, followed by phlebitis and pyæmia.

8. That infiltration of urine, after lithotomy, must be regarded as a surgical curiosity.

It will thus be seen that whether we examine cases of lithotomy on the living or dead, the same conclusions will be arrived at—that an ordinary-sized stone cannot be extracted through an ordinary-sized prostate by means of a limited incision and subsequent so called dilatation without complete rupture of the gland and its capsule. I may here remark that Professor Ellis has for a great many years past practically de-

monstrated to his class that there is no such thing as dilatation of the prostate, and that what surgeons call dilatation is in reality complete laceration. A subject of very great importance, which has not yet received any attention, is the occurrence of impotence after lithotomy. I have seen several such cases. Impotence results from the laceration of the mucous membrane around and lining the orifices of the ducts, and their subsequent plugging; and my objection to the median operation is founded on this ground, for in it the mucous membrane is usually lacerated in the floor of the urethra, and the prostate occasionally split into halves in the paths of the ducts, thus interfering with their integrity.

The practical conclusions at which I have arrived are—firstly, that when lateral lithotomy is performed the stone ought always to be cut out, and not torn out; and, secondly, that the median operation is not justifiable for the extraction of calculi which are upwards of half an inch in diameter, for if such stones be removed by that process obliteration of the orifices of the ejaculatory ducts, and permanent impotence will ensue. Dr. Richardson has stated, from his enquiries, that the median operation is attended with a larger mortality than lateral lithotomy. In boys lateral lithotomy is so successful that no possible advantage can be gained by substituting for it the median operation, inasmuch as that procedure cannot be accomplished in the child without lacerating the very part we ought to avoid—the floor of the prostatic urethra.

Now for a few words regarding lateral lithotomy. (1) The sound ought to have a very short beak, not longer than one inch. The shorter the beak the greater the facility for exploring the bladder. A sound shaped like a catheter will just as often miss a stone as hit one. (2) I use a rectangular staff made after my own design; the vertical part is short, the handle large, and the groove on the horizontal part is continued upwards on to the vertical piece for one inch. The staff ought to be held with both hands; the lower one resting on the pubes. By this method an amount of fixity can be given to the staff unattainable by any other means. I learnt this procedure from Dr. Henry Dick. (3) Some surgeons use a narrower bladed knife when operating on children to what they do in the adult; but this is manifestly wrong, for inasmuch as the same sized instrument, the left forefinger, is introduced in either case, there ought to be no difference in the size of the knife. Most of the accidents that have occurred in connexion with lithotomy have arisen from a difficulty experienced in getting into the bladder in children. This has taken place from using a very narrow-bladed knife in order to make a small internal incision into the bladder; the result being that the opening made

was totally insufficient to admit the forefinger, the bladder therefore being pushed back into the pelvis. (4) In children I never use a foreceps, but extract the stone with the forefinger only. It can be made to slide out of the bladder by simply pressing on it. Another way I have, if I experience any difficulty, is to get the left forefinger under the stone, press it firmly against the bladder at the same time that the right hand is placed above the pubes. In this way the left forefinger can make the stone slip out. For adults I use a simple open polypus forceps. It will never let a stone escape. (5) The external incision ought to commence low down; by this means we wound less vascular parts, and there is but little bleeding—all-important in infirm old men. (6) The internal incision ought to be free, for there is no such thing as dilatation of the prostate. The bladder must be cut into, not merely pierced. (7) I have never used a tube after lithotomy ; for the best of all reasons, that I have not seen its usefulness in the practice of others, and I know that a man has lost his life through its employment.

The above conclusions I have arrived at from carefully watching the practice of other surgeons, from my own experiments and investigations, and, lastly, from my own experience, based on thirty-one cases of lithotomy and lithotrity, all of which have recovered except two.—*Lancet, Dec.* 15 *and* 23, 1871, *pp.* 846, 882.

---

## 66.—CASES OF STRICTURE TREATED BY A NEW WEDGE DILATOR.

By Berkeley Hill, Esq., University College Hospital.

The accompanying woodcut depicts a modification of the principle adopted for Holt's, Perreve's, and Richardson's stricture dilators, which has been used with success by Mr. Berkeley Hill in University College Hospital. The two following cases are selected from a series :—

*Case* 1.—C. J., aged thirty-nine, admitted Sept. 4th, 1871, formerly in the Marine Artillery, has suffered from stricture for many years, and occasional attacks of retention of urine during the last ten years. Has been in hospital in a provincial town; but the surgeons under whose care he has been for the last six years at different times have not been able to introduce an instrument into the bladder. In despair he has come to London for further advice. On admission, the patient is thin, and looks much older than he really is. He is constantly agonised with desire to pass urine; this escapes in drops, or, if he forces

violently for a few seconds, in a stream. The urine is dark, offensive, and one-fourth is ropy pus. In the perineum a mass as large as the finger, and apparently as hard as wood, occupies the middle line. There is no swelling of the superficial parts, and the prostate is not enlarged. During many trials with bougies of various sizes and kinds, and the involuntary explorations of old false passages, on two occasions a fine silkworm gut bougie was got through the stricture, to the slight improvement of the flow of urine. These attempts occupied more than a fortnight, in which time the patient's sufferings were treated by warm baths, rest in bed, suppositories, and subcutaneous injection of morphia.

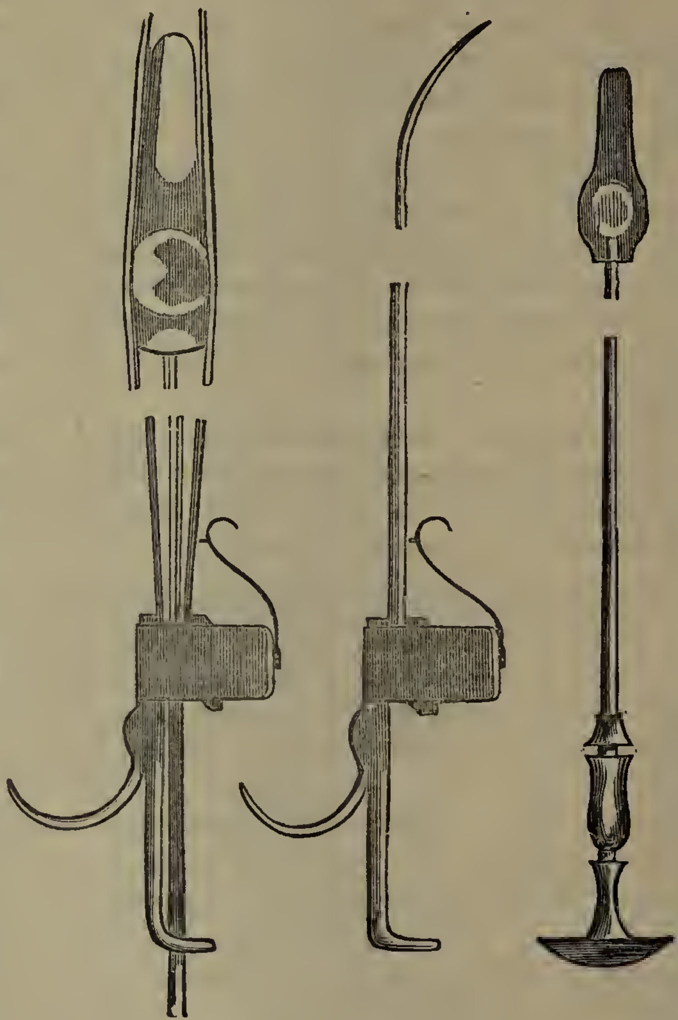
On Sept. 22nd, Mr. Hill succeeded in passing a No. 2 silver catheter under chloroform. This was tied in, and replaced the next day by a flexible one. In a few days, when the irritation had subsided, the patient was again put under chloroform and the dilator passed, a No. 13 catheter being immediately introduced, tied in, and the urine drawn off. No reaction took place.

The patient was trained to pass an instrument for himself, and was discharged on October 6th, passing No. 12 easily, his urine being quite clear and free from albumen. He lay all night without passing urine.

*Case* 2.—A groom, aged twenty-six; admitted Oct. 5th, 1871. Nine years before he had contracted traumatic stricture by falling astride a bar. For the pain, bleeding from the urethra, and inability to pass water, he was treated at Guy's Hospital. Five years later, retention having again occurred, he went to Guy's, and his stricture was again dilated by the daily passage of instruments. After each discharge from hospital he neglected the injunction he had received to pass a bougie from time to time. When admitted to University College Hospital, a large, hard mass, was found in the perineum, and the patient was passing urine every quarter of an hour; urine clear. After some difficulty Mr. Hill passed a No. 1 flexible catheter through a stricture four inches and three-quarters beyond the meatus. This was tied in; and on the 12th the stricture was easily split to No. 14, and a No. 12 catheter tied in for twenty-four hours. In a few days the patient could pass a catheter for himself, make a good stream, and was discharged with the strict injunction not again to neglect the occasional passage of a bougie, No. 10, which was given to him.

These cases have been selected from several others treated by the dilator as samples of copious induration of the urethra giving way instantaneously before the wedge on the exertion of

very little force. As will be seen by a glance at the drawing, the instrument consists of a split sound which equals the calibre of a No. 2 or No. 3 catheter. The halves of the split sound can be separated by passing between them a wedge fixed on a slender stem. The wedge is prevented by two dovetail grooves at its hinder part from leaving the sound, which acts as the guide during the passage of the wedge down the urethra. When the instrument is used, the urethra is first explored by an olive-headed bougie, graduated on its slender stem, by which the situation of the stricture is ascertained. The split sound is next passed along the urethra till the beak reaches the bladder. The wedge is then inserted between the blades, and pushed swiftly down till it has passed the stricture. It is then withdrawn, the blades of the sound fall together, and can be removed easily. A full-sized No. 12 or No. 13 catheter is at once passed into the bladder to draw off the urine, and may be taken out out or tied in for twenty-four hours. On the third day a No. 12 or No. 13 bougie is passed, and again every other day for a week, while the patient learns to use the instrument himself, and not to let more than a fortnight pass without doing so. The instrument is adapted for the same cases as Mr. Holt's—namely, strictures situated in the bulbous portion, not in the penile part, where the division of a stricture should be a clean cut, in order that a cicatrix may as little as possible block up the erectile tissue and interfere with erection.



The advantages claimed for this instrument are: simplicity of construction; the central guide of Holt's instrument is not needed, hence the split sound can be passed through narrower

strictures. Next, and chiefly, diminution of resistance, and, consequently, the more immediate application of the rupturing force—the wedge—to the impediment to be overcome. The force needed to push Holt's dilator is sometimes very great, and the attempt has been abandoned, or the instrument has broken, even in skilful hands, from this cause. Much of the force is expended on the continuous friction outside the tube along the split sound, and inside along the guide. In the wedge dilator the friction surface is reduced to two dovetail grooves, which together do not exceed half an inch. For this the force required is so small that one hand suffices to overcome the resistance.—*Lancet, April* 6, 1872, *p.* 466.

---

## 67.—ON THE TREATMENT OF RETENTION OF URINE IN IMPERMEABLE STRICTURES OF THE URETHRA.

By Dr. P. A. O'Connell, late Medical Director of the "Ninth Army Corps," U.S. Army, &c.

[It sometimes happens that owing to carelessness, or fear of pain, a man lets a gradually contracting stricture go untreated so long, that he suddenly finds himself unable to void urine at all.]

When the surgeon sees the case the catheter is resorted to of course, but the attempt to pass it fails. Instruments of different sizes and material, perhaps, are tried, but in vain; and after prolonged, patient, and skilful labour over the case, the surgeon finds himself unable to effect an entrance through the stricture by any means at his disposal. The stricture is an impermeable one; baths and such aids to treatment have failed to break up the embargo; the patient is suffering intensely from the retention of urine; no time is to be lost, and what is to be done?

In two cases of this kind which have come under my care I have been able to relieve the patient upon the principle of suction; and the simplicity of the method, as, also, its advantages if, upon further trial, it be found to be of universal application, prompt me to make it public.

In the first case which presented itself the gentleman had suffered from retention of urine on account of stricture before, and, consequently, was not easily alarmed. He was always intending to submit himself to treatment for the cure of the stricture; and this *in*tention became very determined and positive during his attacks of *re*tention; but relief from immediate danger made him forget his resolution, and he would always postpone action. When I saw him, however, the usual means of relief had failed. Baths, opiates, and the catheter

had been resorted to by a medical gentleman of skill to whom he had applied for assistance, but all in vain, and his sufferings were very severe. I made careful attempts to pass instruments, but without success. The other means of relief that suggested themselves to my mind had failed already, and the sufferings of the patient required that something should be done immediately.

Having upon my office table an india-rubber hand-syringe, consisting simply of a rubber pouch or ball, with a hard rubber stem to it, that I generally used as a part of Politzer's apparatus for inflating the inner ear, it occurred to me that it might be made use of as an exhauster—a suction instrument,—and that by this meams, perhaps, the stream of water could be started. Acting upon the idea, I took a catheter of medium size, made a perforation in its extreme end, and passed it *down to* the stricture; then squeezing the rubber pouch so as to drive out the air, I connected it by means of a short piece of india-rubber tubing with the catheter already in the urethra, and allowing it to expand gently, instructing the patient at the same time to make a gentle effort, *and only a gentle effort*, to pass his water, I had the satisfaction of learning that the experiment had become a success, aud that the man was relieved.

As this was merely a transient patient whom I had not seen before and did not see again, I do not know what became of him afterwards. He stated, while in agony, that he would return on the following day to place himself under treatment for his stricture, but, as in the previous instances he had told of, no doubt relief brought with it carelessness, and I saw him no more.

In the second case in which I tried this method, the man was a reckless, careless liver; also a procrastinator, for whose procrastination, however, fear of anything bearing the name of "operation," perhaps also a chronic scarcity of money, acted, to a great extent, as cause.

The use of the india-rubber bulb, exhausted and then fixed to the catheter after its introduction *as far as the stricture*, succeeded in this case also. Once afterwards, when the passage had become closed again, and his experienced effortsto relieve himself had failed, so that the case presented apparently the same necessity for the use of the suction apparatus as before, percussing the bladder after all other efforts had failed (this was done for the purpose of ascertaining the extent of the distension) alone started the flow, and he became relieved without any further operation. This latter occurrence suggested the probability of the retention having resulted from some slight mechanical obstruction inside the stricture which became dislodged by the percussion, and thus allowed the water to pass. Whether such a condition of things would interfere

with the success of the method of treatment by the suction conducing rather to retain the obstruction *in situ*, must be the subject of further experience.

The attention of surgeons is called to this method of relieving retention of urine from impermeable strictures, when other methods have failed, with the hope that further careful experiment will develop its value fully.

No expensive apparatus is required, a simple catheter perforated at its extreme end (although a hollow tube without side openings through which the lining of the urethra can be acted upon would be better), and a hollow india-rubber ball that may be connected with the catheter by means of a short piece of india-rubber tubing, is all the paraphernalia required; and I doubt not that its use will relieve much suffering, and give time for subsequent calm and deliberate treatment for the permanent cure of the stricture itself.

Introduce the tube, or catheter, as far as the stricture; let the suction be gentle and continuous, and let the patient assist, by a very easy effort, to void urine during the operation.

It is possible that a common syringe of any kind might be used for this same purpose in place of the india-rubber bulb, but as more is expected from the persistence of the force working continually in the right direction than from its violence, there seems to be good reason for adhering to the original method. A common syringe, used with rashness, is capable of doing injury in cases of this kind.—*Lancet, March* 2, 1872, *p.* 286.

---

## 68.—VERTEBRATED PROSTATIC CATHETER.

By Dr. T. H. Squire, Elmira, New York.

[Formerly a rigid metallic instrument of large curve was the favourite instrument in cases of prostatic disease. There are, however, many cases in which only men of the greatest skill can introduce such an instrument; and we believe of late years that a gum-elastic, or india-rubber catheter is far more frequently employed.]

In the consideration of the subject of catheterism, in prostatic retention, one fact occurs, possessed of the utmost significance, namely: *the seat of embarrassment and failure is, always, in the deeper part of the canal, very near to the bladder.* Why is it that the rigid catheter invariably passes easily through the anterior three-fourths of the canal, and so frequently meets with impediment or failure in the posterior or vesical fourth? The reason is: The anterior part of the urethra being perfectly flexible, adapts itself to the curve and direction of the instru-

ment, and there is no resistance, whilst the deeper part of the canal, being rigid and fixed, cannot adapt itself to the form and direction of the catheter, and as soon as the axis of the instrument ceases to be in correspondence with the axis of the urethra, that moment advancement is difficult or impossible. If the discrepancy be considerable, and too much force be used, a perforation of the canal is sure to be the result. The gum-elastic is better than the rigid catheter, for, being somewhat flexible, it has a limited power of adapting itself to the dicection of the rigid urethra. But both instruments are faulty in this, that they cannot accommodate themselves to the direction of that part of the canal which is rigid and often tortuous in its course. In this connection the following question arises: If a rigid instrument will go with ease through a perfectly flexible canal, why will not a *perfectly flexible* instrument pass with ease through a rigid canal? Inasmuch as the canal, in the one case, readily conforms to the instrument, the instrument, in the other case, ought readily to conform to the canal. When the correctness of this theory had become firmly established in my mind, I determined to construct an instrument, that, with every other essential requisite for a prostatic catheter, should in its vesical extremity possess this quality of *perfect flexibility*. What, then, are essential requisites for a prostatic catheter? They are as follows:—

1. It should have a good calibre for the flow of urine.
2. It should have a smooth external surface, that it may glide in the canal without friction or pain.
3. The vesical portion should be perfectly flexible and floating, that the beak may readily follow the curves of the rigid portion of the urethra.
4. It should have linear or longitudinal stability, that the propulsion of the hand at the pavilion may be transmitted without loss to the beak.
5. It should possess transverse stability, that its calibre may be preserved.
6. It should have such strength as to make it secure against accidental breaking in the canal.
7. The flexibility of the vesical portion, perfect in degree, should be limited in extent, that the instrument cannot double on itself in the canal.

In order to secure all these requisites, the instrument should be made of silver or some other hard material, and the flexibility of the vesical portion obtained by means of a series of perfect joints or articulations, giving it a vertebrated appearance.

The rigid portion, which may be called the shaft of the instrument, is from eight to nine inches in length, and number ten in

size. At its outer extremity it resembles an ordinary silver catheter. The opposite extremity is ovoid in form, and has a central opening reduced to number five in diameter. The latter extremity or floating part of the catheter, is composed of from twelve to sixteen vertebræ, or joints, including the beak. Each vetebra is one-fourth of an inch in length, and bears some resemblance to a common teacup, having an open mouth surrounded by a thin, smooth, well-rounded lip, and an opposite extremity which is precisely like the ovoid extremity of the shaft. The beak has an open mouth like each of the vertebræ, and in other respects is similar to the beak of an ordinary catheter. The shaft and the various sections of the floating part are held in loose apposition by means of an internal chain and rod—chain in the floating part, and rod in the shaft—the two forming a continuous ligament of connection. The end of the chain is secured within the beak, and the rod terminates with an eye at the pavilion, and through this eye passes a small screw-bolt, the thumb-piece of which is one of the rings or ears, at this extremity of the instrument. Or the rod may be secured in the following way: The rod can be made to terminate in a right angle with a ball at its end, the latter is then passed through a fine slot very near the end of the tube, and the short arm given a quarter turn and secured by a cap having a bayonet slot in the side, which cap is slipped on the end of the tube. This cap should have a slot in the opposite side to admit the neck of a stationary ball.

The exact length or tension of this internal ligament is a matter of the utmost importance, for upon this depends the essential virtue of the instrument There is but one degree of tension that is appropriate. If the tension be too great, the flexibility of the floating part is seriously abridged; if it be too slack, the tubal integrity of the instrument is lost. In order to be exactly right, the chain should be as lax as may be, and not allow the ovoid extremity of any vertebra to escape from the mouth of its fellow. In this condition perfect and delicate flexibility is secured, and, at the same time, the integrity of the tube is preserved.

Another feature in the construction of the instrument requires special comment, namely: the precise form of the ovoid extremity of the shaft and of the vertebræ. If the contraction of this extremity be too abrupt, approaching a spheroid form. it is objectionable; and on the other hand, if it be not sufficiently abrupt, if it be too conical, it is equally at fault. The smaller extremity of an ordinary hen's egg supplies an appropriate illustration of the form required.

About one year ago, I had some experimental instruments made, and with the aid of quite a number of professional friends,

began to submit the catheter to practical testing in typical cases of prostatic retention, and, although the information thus sought has been slow and difficult of acquirement, nevertheless, the results, thus far, have been exceedingly gratifying, as the following cases will show :—

*Case* 1.—Reported by Dr. Caro, of New York. M., æt, 60, corpulent, average weight 300. Temperament sanguine, temperate and healthy. Six months ago he commenced to have difficulty in passing urine. Upon examination, I found he was troubled with prostatic enlargement. I tried a No. 8 elastic catheter, but could not pass it. A common silver catheter of the same size was passed with great difficulty, producing laceration of the prostate, causing considerable hemorrhage, distress, and delay in using any other instrument. I obtained from Messrs. Stohlman and Co., one of your vertebrated catheters No. 8.

After four days I attempted a second catheterism. To my great satisfaction, and with very little difficulty, I succeeded in passing it to the bladder, affording immediate relief to my patient by drawing two quarts of bloody urine. Four days of practice made him acquainted with the manipulation of it, and ever since he has been enabled to relieve himself from the distress of want of free micturition.

*Case* 2.—Reported by Dr. Walter Booth, of Boonville, New York. Was called four miles to see a man, aged 55, and found him suffering from retention of urine. There was great distension of the bladder and very severe pain. At once attempted to introduce the common silver catheter, but failed to reach the bladder. Returned for the vertebrated instrument, and within one hour was by the side of the patient again. The instrument passed at once and without the least difficulty, and, of course, relieved him of his sufferings. I continued the use of the new instrument five or six days, when the patient was discharged cured.

*Case* 3.—Reported by Dr. J. L. Stewart, of Erie, Pa. The vertebrated catheter was used with great satisfaction ; succeeded in a case where others had failed with the common instrument.

*Case* 4.—Reported by Dr. Wm. E. Johnson, of Waverley, New York. Mr. ——, æt, 56. After a long ride on the cars, had retention of urine, severe pain, tenesmus, &c. Employed the silver catheter, which with difficulty was introduced and with very unsatisfactory results; but little urine escaped, and upon the withdrawal of the instrument, as well as during its presence in the canal, blood escaped.

In this dilemma the vertebrated catheter was used, and between two and three pints of urine drawn off with no pain, and very little blood following.

Next day used the catheter three times, and in each instance with ease and satisfaction. Patient then used the instrument himself, till the power to urinate returned.

*Case* 5.—Reported by Dr. F. Abbott, of Elmira, New York. Was called to a patient, aged 60 years, suffering from an attack of prostatic retention of urine. Failing after repeated efforts with the usual gum-elastic and the rigid catheters, recourse was had to the vertebrated prostratic catheter, which entered the bladder with perfect easc, and three pints of urine flowed away. Patient afterwards used the instrument himself, till the natural function was restored.

*Case* 6.—Reported by Dr. J. K. Stanchfield, of Elmira, New York. Retention of urine in a man 57 years of age. Unable to pass a common silver catheter, vertebrated catheter readily, and without pain, entered the bladder as if by its own gravity.

*Case* 7.—Reported by Dr. William Woodward, of Big Flats, New York. W. R., farmer, æt. 66; after a long ride, was seized with complete retention of urine. He was speedily relieved by the use of the common silver catheter; but, requiring the operation to be repeated more than twice a day, the neck of the bladder became inflamed, and the introduction of the catheter was attended with pain. On the sixth day of his illness the vertebrated catheter was used, and was passed with much less difficulty and pain than attended the use of the common catheter. On the tenth day the patient began to use it himself, and he continued so to do as long as an instrument was required.

*Case* 8.—Reported by Dr. H. Lyle Smith, of Hudson, New York. Patient 76 years of age, with prostatic retention of urine. The common gum-elastic catheter was used without difficulty by the surgeon; but the patient, in attempts to introduce it himself, could not succeed. At length the vertebrated catheter was substituted (Sept., 1870), and its introduction was attended with so little pain, that the patient used it himself for a long time with the greatest satisfaction. Death finally occurred from gradual exhaustion. The new catheter was employed as long as any instrument was called for.

*Case* 9.—Reported by Dr. Hutchison, of Brooklyn, New York. Dr. H., in January, 1870, was called as counsel to a case of retention of urine from enlarged prostate, and being unable to go, sent his assistant Dr. Wilson. The two physicians who had charge of the case, one a gentleman of large experience, had tried ineffectually for an hour or two to enter the bladder. Dr. W. introduced a No. 7 or 8 catheter, with but little difficulty, and subsequently the patient tried to introduce the catheter, but was unable to do so. He (the patient) did, however, introduce the vertebrated catheter with the

greatest ease, and continued to do so three times a day as long as it was required.

*Case* 10.—Reported by Dr. Hutchison, of Brooklyn, New York. Mr. ——, æt. 60, had prostatic retention, which the attending physician was unable to relieve by the catheter after two hours' effort. Dr. Wilson then saw the case Jan. 1871, for Dr. Hutchison, and introduced quite readily a No. 8 silver catheter. On the following day Dr. H. saw the patient, and introuced a No. 8 readily, and twenty-four hours later he requested the patient to try the vertebrated instrument. The patient was timid and agitated, but passed the catheter to the prostate, where it stopped; the *slightest* push, however, by Dr. H. caused it to enter the bladder. It was subsequently introduced by a son-in-law of the patient without any difficulty.

*Case* 11.—Reported by Dr. Caro, of New York. A healthy man, æt. 81, weighing 250, had retention of urine from enlarged prostate gland. Tried a No. 8 silver catheter to relieve him of the accumulated urine in the bladder, and probably, by force, could have passed it, but finding an obstacle at the prostate, and the patient suffering great pain, the instrument was withdrawn, and a No. 8 vertebrated catheter was passed without the slightest inconvenience, relieving him of about three pints of brown-coloured urine. Afterwards the nurse accomplished the operation regularly, and always with ease and great satisfaction to the patient.

*Case* 12.—Reported by Dr. May, of Corning, New York. Young man suffering from distended bladder, with inflammation of the bowels. Attempted to relieve the retention with No. 9 silver catheter, also with No. 6; but did not succeed with either. With the vertebrated instrument, No. 10, had not the least difficulty in reaching the bladder. Nearly two quarts of acrid urine were drawn, to the great relief of the patient. The instrument was used once afterwards, and then the natural function was restored.

*Case* 13.—Reported by Dr. May, of Corning, New York. Having occasion to tap a man for ascites, and desiring, as a precautionary measure, first to evacuate the bladder, the vertebrated prostatic catheter was used, which reached water with the greatest facility, inflicting no pain whatever upon the patient.

*Case* 14.—Reported by Dr. Bates, of New Lebanon, New York. A mechanic fell from a building, was badly injured, kidneys wounded, and bladder filled with urine and blood. Tried the ordinary catheters, but could not get into the bladder. Then tried the vertebrated instrument with perfect success. Had to use it many times. Patient recovered. Attributed the life of the patient to the use of this catheter.

*Case* 15.—Reported by Dr. Smith, of Hornby, New York. Was called to N. O., of Orange, N. Y., May 5th, at 4 o'clock p.m., who was suffering from retention of urine of twenty-two hours' standing. Had suffered much pain for nineteen hours. The case was so evident, and pain so severe, I concluded to lose no time with fomentations, &c. Passed a gum-elastic catheter, No. 8, deep into the urethra, but met with resistance near the prostate. The instrument would invariably turn to the right, showing the channel to be tortuous and uneven. Carried my index finger in the rectum, and found prostate gland very much enlarged, heat of parts much above natural standard, and a good deal of soreness. I pronounced the case acute prostatitis with hypertrophy. By retaining my finger against the membranous portion of the urethra, or nearest portion of prostate gland, I succeeded with much difficulty in reaching the bladder, and drawing off about twenty-four ounces of high-coloured urine, which of course relieved the patient; gave opium.

May 6th. Called at M., and found the patient suffering from retention as on the day previous, but not so severely. Introduced catheter, but with a little less trouble, and drew sixteen ounces of urine.

7th. Was called very early in the morning with message that patient was suffering very much. I saw him at 7 o'clock a.m., and attempted to introduce catheter as before, but failed; heat in the rectum intense. Gave an opiate, and sent for an experienced and skilful physician from Monterey, who reached the patient at 10½ a.m. He made a persevering effort to introduce various instruments, but failed to reach the bladder. Ordered nauseating medicines, morphia, hip-bath, &c. At 2 o'clock p.m., I succeeded in reaching the bladder with a No. 8 silver catheter, with a long, deep-sweeping curve, and drew 1½ pint of urine.

8th, 9th, 10th, and 11th. I introduced gum-elastic catheter with much difficulty, generally without the stilet. Could pass the gum catheter with less difficulty than the silver; although the silver instrument was a remarkably good one for the occasion, as it was long and deeply curved. The patient, at every attempt to pass the catheter, became very much exhausted, and I could see he bore each successive operation with more difficulty, as he grew more and more exhausted, his strength constantly failing.

12th. Called on Dr. May, of Corning, and through his kindness obtained a vertebrated silver catheter. On exploring the urethra, found the same zigzag course of the canal. Could with some difficulty reach the prostatic portion, but then came the obstruction as before, and, as it worried my patient so

much, I threw the old instruments all aside and attempted the introduction of the new catheter. It being about three sizes larger than those I had before used, I used but little force, giving the passage sufficient time to dilate, but, in less than one-half the time required before, I had the satisfaction of reaching the bladder, followed by a copious flow of urine. Here was success I had hardly dared to expect. At every attempt to introduce the other instruments more or less blood was sure to flow. But in introducing this new instrument no hemorrhage whatever followed. The patient was extravagant in delight. In no case afterwards could I induce him to let me pass any other instrument. I introduced this twice a day, for three or four weeks. Since that time to the present date, he has been able to pass urine with more or less difficulty without the aid of any instrument whatever.

Occasionally when using the catheter I could detect small calculous deposits. Since he has been able to void urine naturally, more or less gravel has escaped with the water.

*Case* 16.—Reported by Dr. Gere, of Chemung, New York. G. C., æt. 70. In the month of March he was taken with complete retention of urine. A physician was called, and the common catheter was used with considerable difficulty and pain. The vertebrated catheter was afterwards tried, which proved so easy of introduction that the patient himself has since used it regularly, and is still obliged to use it twice daily. The prostate gland is enormously enlarged, almost entirely filling the pelvic cavity, and seriously obstructing the passages from the bowels.

*Case* 17.—Reported by Dr. Purdy, of Elmira, New York. Was called in counsel to see Mr. E., æt. 70; found him in a state of extreme suffering from prostatic retention. Having the vertebrated catheter with me it was used at once, with the utmost facility and freedom from pain. A relation of the patient afterwards used the instrument, till the man was able to urinate.

The testimony of these cases confirms the correctness of the theory or principle embodied in this new catheter. It settles the question that, in the introduction of a catheter for the relief of prostatic retention, the *urethral walls*, and not the surgeon, should guide the beak of the instrument. I do not hesitate, therefore, in pronouncing this new catheter *a real and very important improvement* in the art of modern surgery.

My very grateful acknowledgments are due to those members of the profession who have kindly assisted me in testing the practical merits of this new catheter.

The vertebrated prostatic catheter may be obtained from J. H. Gemrig, No. 109, South Eight Street, Philadelphia.—*American Journal of Medical Science, Oct.* 1871, *p.* 393.

---

### 69.—CATHETERISM; SYNCOPE; EMBOLISM; DEATH.

By FRANCIS D. LYS, Esq., Bere Regis, Blandford.

The following case, though happily rare, shows the necessity for increased caution even in the slightest surgical procedure upon those patients in whom we have reason to suspect any cardiac lesion.

J. S., a very stout man, aged sixty-two, had always enjoyed good health till within the last few months, during which he has suffered from atonic dyspeptic symptoms, for which he consulted me on October 5th, and again on the morning of the 8th, when he was somewhat better, though his pulse was weak, and he complained of shortness of breath when walking up-hill. While driving home he wanted to pass urine, but was prevented, by the presence of a female. On his arrival he had lost the power to relief himself, and came to me again in the evening.

I passed a No. 10 catheter for him whilst in the recumbent position, and, having done so, he sat up in order that he might the better empty his bladder, which contained about fifteen ounces—certainly not more. He now complained of feeling faint, which induced me to lay him down again and to give him stimulants. In spite of this, he remained almost pulseless for more than an hour, and it was quite two hours before he sufficiently rallied to admit of his removal from my surgery.

Oct. 9th. He has passed urine twice without difficulty. He has been sick several times. His pulse is very weak and intermittent. I gave him milk, beef-tea, and brandy mixture, and ordered a sinapism to the epigastrium.

10th. Pulse scarcely perceptible. He had several convulsive attacks, and died in the evening.

I regret that no post-mortem examination was practicable. I believe, however, that it would have revealed that the man had a fatty heart, and that during the syncope caused by the shock of passing the catheter a clot formed either in the heart or aorta, which prevented his recovery. Nobody accompanied him except a little boy to drive him; it is therefore fortunate that he rallied sufficiently to be removed home.

From this case, it seems to me that when there is reason to suspect any weakness in the heart, it is advisable to enjoin the recumbent posture till after the removal of the catheter, if not for a short time longer.—*Lancet, Nov.* 25, 1871, *p.* 746.

## DISEASES OF THE EYE AND EAR.

### 70.—ON THE TREATMENT AND ORIGIN OF PURULENT OPHTHALMIA IN NEW-BORN CHILDREN.

By R. Liebreich, Esq., Ophthalmic Surgeon to St. Thomas's Hospital.

[The treatment recommended by Mr. Liebreich, consists]

First, in careful cleansing; secondly, in the local application of cold; and, thirdly, in cauterisations with mitigated nitrate of silver (one part of nitrate of silver and two parts of nitrate of potassium fused together). Allow me now to speak of this in a somewhat elementary and detailed manner. As regards the cleansing, it is above all necessary to explain to the attendants the importance of it, for, ordinarily, the fear of injuring the child prevents them from properly opening its eyes in order to remove the secretion. I do not recommend syringes, so generally used, for cleansing—first, because they are dangerous to the attendants, who, in using them, may easily have some of the contagious matter spattered into their eyes; secondly, because by this method the secretion is not completely removed, even after pouring much water over the child. A fine sponge, if you can rely upon it being kept clean, or, if not, small pieces of moistened linen rag, are preferable for effectually cleansing the conjunctiva.

The application of cold, if made in a careful and suitable manner, is of great assistance in the treatment. In the mildest form of the disease this application alone may even effect a cure in a few days. It is then only necessary to apply, for several hours a day, small linen rags, moistened by being dipped in cold water, changing them constantly. In the more serious cases, on the contrary, when there is much swelling, redness, and heat in the eyelids, and a copious discharge of thick yellow purulent secretion, it is necessary to apply, day and night, without intermission, small rags, previously placed upon ice, and to renew them continually. Later, when the elevated temperature begins to fall, the applications may be discontinued during the night, and gradually reduced, according to the course of the disease. In order to prevent the child from taking cold, it is necessary that the rags should be of a size merely to cover the eyelids without touching the bridge of the nose, and not to make them too wet.

We now come to the real curative treatment—that is, the cauterisation. It should only be done with mitigated nitrate of silver. The eyelids must be reversed one after the other, and, after being carefully cleansed, touched with the caustic, which must be passed over all the swollen and red part of the

mucous membrane. Before replacing the eyelids in their natural position, it is necessary to neutralise the free nitrate of silver by a drop of salt water. For the first few days only of the disease we may restrict the treatment to the application of cold, and then commence the cauterisations; repeat them once a day, never more frequently, and, after an evident reduction of the disease, once every two or three days. It is important not to repeat the cauterisation until the scar of the previous one has disappeared.

To drop weak solutions of nitrate of silver into the eye is not advisable, even in the mildest forms; for the graver forms it is completely insufficient. Cauterisation with pure nitrate of silver ought never to be used, neither in this nor in any other disease of the conjunctiva, as it is impossible to limit its effects. The slightest touch with pure nitrate of silver, in fact, produces a strong cauterisation, not only not limited to the surface of the mucous membrane, but attacking the subjacent connective tissue. The cicatrisation which is the result of such cauterisations produces a permanent irritation of the eye, which cannot be removed by any possible means. None of the other known caustics can replace the mitigated nitrate of silver in the treatment of purulent ophthalmia. Let nobody avoid the trouble of preparing a pencil of nitrate of silver himself if he has not a suitable one already prepared at his disposal. The sole difficulty lies in procuring an iron mould, into which the mixture of one part of nitrate of silver and two parts of nitrate of potash is to be poured after having been melted over the fire. This difficulty could be avoided by immersing an iron wire repeatedly with one end into the melted mass until, on cooling, a sufficiently thick layer of caustic adheres to it. That is the procedure which I formerly recommended for caustic probes in cases in which it is desirable to cauterise with a very fine point or in a very narrow canal (lachrymal canals, lachrymal sac, ciliary roots, &c.).

In the most difficult cases, cleanliness, cold, and cauterisation are sufficient, and enable you to form a good prognosis. The case is, however, different when children come under your treatment, after a more or less great part of the cornea has already been destroyed, the iris projecting, the capsule of the lens injured; here the prognosis depends entirely, in the individual case, on the existing destructions. Ulcers of the cornea of no great extent, and not too near to perforation, allow of a still favourable prognosis; but it becomes more unfavourable already, when perforation has taken place, and the iris projects. There is a decidedly bad prognosis if almost the greater part of the cornea had been destroyed before the child came under your treatment.

In such grave complications, it is sometimes desirable to cut off the projecting iris, or to let out the lens. But I advise you generally to abstain from those operations, which require considerable experience and practice. Direct your attention, then, exclusively to the quick cure of the affection of the mucous membrane, whatever the complication may be which neglect and false treatment have caused. You will be astonished to see how quickly even the gravest complications come to a comparatively favourable cure, as soon as the possibility of it has been given by the removal of the affection of the mucous membrane. I have shown you an instance of it in a child, in one of our previous meetings; it was brought as an out-patient, after it had suffered for more than three weeks from a very high degree of purulent ophthalmia, and had been treated during all that time with warm milk.

If you inquire into the cause why the use of warm milk or of moist heat in general, in spite of its evident mischief, and in spite of the repeated warnings of ophthalmologists, could have gained ground in all countries, amongst the public as well as amongst general practitioners, you will find that it originated in an erroneous supposition on the nature and origin of the affection.

It has always struck me with what haste relatives or nurses declare, as soon as they bring a case of purulent ophthalmia for treatment, that the affection is due to having caught cold. The same will be told you if you have to examine grown-up people, where you will find the results of an ophthalmia from which the individual has suffered immediately after his birth. In addition to the erroneous supposition of having caught cold in the eye, comes the wrong principle of treating that cold by warmth; and these errors have preserved themselves for generations, in spite of all the opposition against them. Let us, therefore, first of all, understand the pathogenesis, of which, in my opinion, there is not the slightest doubt.

You may accept it as proved that any pathological secretion of any mucous membrane, be it catarrhal or purulent, brought into the conjunctival sac, will cause a pathological condition of the conjunctiva, which will, on the one hand, according to the nature of the secretion, and on the other hand, according to the peculiar disposition of the individual, assume a variety of appearances. On going through the maternal passages, the eyes of the child are exposed to inoculations from two quite different sources. They are—first, gonorrhæal secretions from the urethra of the mother (this causes the most formidable cases of purulent ophthalmia, but only an extremely small percentage of the cases are due to that cause); by far the greater majority of cases are due to inoculation from the second of the

two sources—namely the secretion due to the increased functional activity in which the maternal passages are in the last month of pregnancy. According to the nature the secretion had taken in a given case, and according to the quantity of it introduced into the eye of the child, we shall see a series of affections ranging from simple conjunctivitis up to those forms of purulent ophthalmia which in no way are different from those caused by gonorrhœic secretion.

Whatever cause may have produced the disease, and whatever degree it may have reached, let me repeat to you it must be cured without leaving any serious pathological changes by a rational treatment, if early directed against it. Let us hope that rational principles in regard to this disease will soon be so generally spread that blind asylums will cease to be filled, as is now the case, by individuals who have lost their sight by purulent ophthalmia.—*Medical Times and Gazette, December* 23, 1871, *p.* 763.

---

## 71.—ON STRUMOUS OR PHLYCTENULAR OPHTHALMIA.

By Dr. H. R. Swanzy, Ophthalmic Surgeon to the Adelaide Hospital, Dublin, and Assistant to the late Prof. Von Graefe.

This disease is called strumous, because of the condition of the general health which it commonly attends; and phlyctenular, because the little vesicles, which form on the conjunctiva or cornea, as the case may be, are termed phlyctenulæ. It is a very common disease—a large proportion of the out-patients of this department of the Hospital labouring under it. Its most common victims are children between the ages of three or four, and eleven or twelve; but it is by no means uncommon in still later periods of life. In many of the text-books you will find phlyctenular ophthalmia divided under the two heads of "Diseases of the Cornea," and "Diseases of the Conjunctiva." The arrangement is very absurd, for the affection is the same whether it appear on the cornea or conjunctiva, it is treated in a very similar way in either case, and, moreover, it most frequently attacks both tissues simultaneously, or passes from one to the other. The vesicles, which make their appearance in the disease, vary from the size of a pin's head, or somewhat larger, to very small specks like grains of fine sand, which are distinguished by the naked eye with difficulty, unless when a great number of them occur close together, as is indeed usually the case.

Perhaps the most common form of phlyctenular ophthalmia is that in which the conjunctiva, close to the margin of the cornea, becomes swollen at once place, and there small points,

like grains of sand, make their appearance. There is a good deal of conjunctival and sub-conjunctival injection of vessels in the neighbourhood, and the remainder of the conjunctiva participates more or less in the hyperæmia. At this stage there is often a good deal of tearing and photophobia. The disease may cease here, or, after a few days, we may find a number of very fine phlyctenulæ in the adjacent part of the cornea, giving it the appearance as if sprinkled with powder. If the cornea become affected, the tearing and the photophobia increase.

Another common form of the disease is the solitary phlyctenula, which appears on the conjunctiva as a rather large vesicle; it may be situated at some distance from the cornea, or close to its margin. No matter how many of these may be present, they never become confluent. This form of the affection frequently produces but little irritation of the eye, so long as it is confined to the conjunctiva; when, however, one or two of these phlyctenulæ are situated close to the cornea, circumscribed opacities are apt to be formed in the cornea itself, and we thus have one of the most frequent and severe forms of phlyctenular corneitis, attended with distressing symptoms.

A third form of strumous ophthalmia is the broad phlyctenula, situated on the conjunctiva, usually at some distance from the cornea. It is uneven on its surface, appearing to be formed of an aggregation of small infiltrations. It is much rarer than the two forms I have already described to you, and it is less frequently associated with corneitis; but, on the other hand, it is more apt to strike deep, and produce inflammation of the sclerotic.

Still another form of the disease is termed "fascicular corneitis." Although I have seen almost as many cases of this in Germany as of any of the other forms, still I have not, as yet, met with one in this country, and I shall not, therefore, confuse you by a description of it—my great object being to make you acquainted with disease of the eye as you are most likely to come across it.

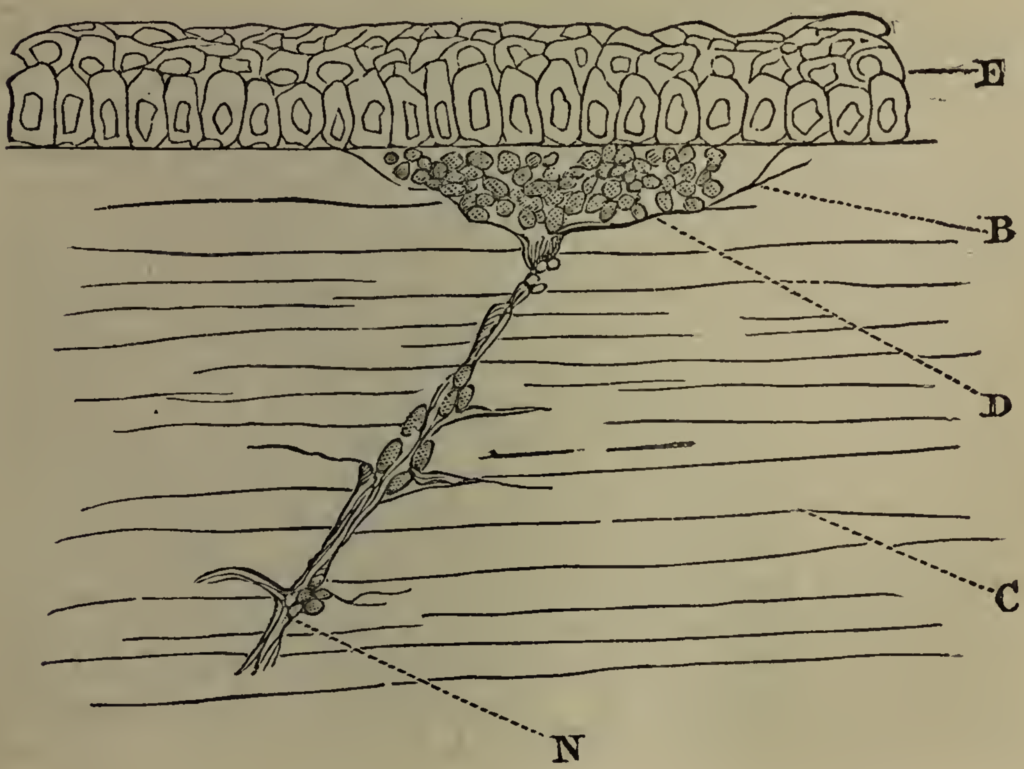
I think you will find all cases of phlyctenular ophthalmia referable to one or other of the forms I have described. They will, of course, present different pictures according to their different degrees, according to the way in which they have been treated or mistreated, and according to the extent to which they have been neglected. Injudicious treatment may alter the natural appearance of any disease, so as to make the diagnosis difficult enough. A common error, against which you must guard, is that of confusing pustular with phlyctenular ophthalmia. The former is most frequently met with as a

sequel to small-pox, and is one of the most destructive affections to which the eye is liable, differing, thus, widely from phlyctenular ophthalmia in its prognosis. Its treatment also is quite different, and pathology will, doubtless, sometime demonstrate, that the two diseases are distinct in their nature.

The greatest danger in phlyctenular ophthalmia is, that when an infiltration of the cornea ulcerates, the process may lead to perforation of that membrane. This is, however, of comparatively rare occurrence. What we commonly have to fear, are opacities of the cornea, left at the places where infiltrations and ulcers were situated. Even mild cases of phlyctenular corneitis, frequently leave opacities behind, which may be recognised with the aid of the oblique illumination. The more severe cases, however, accompanied with intense infiltrations, particularly if these had assumed a purulent character, are apt to leave white opacities behind, which are very disfiguring, and very detrimental to vision.

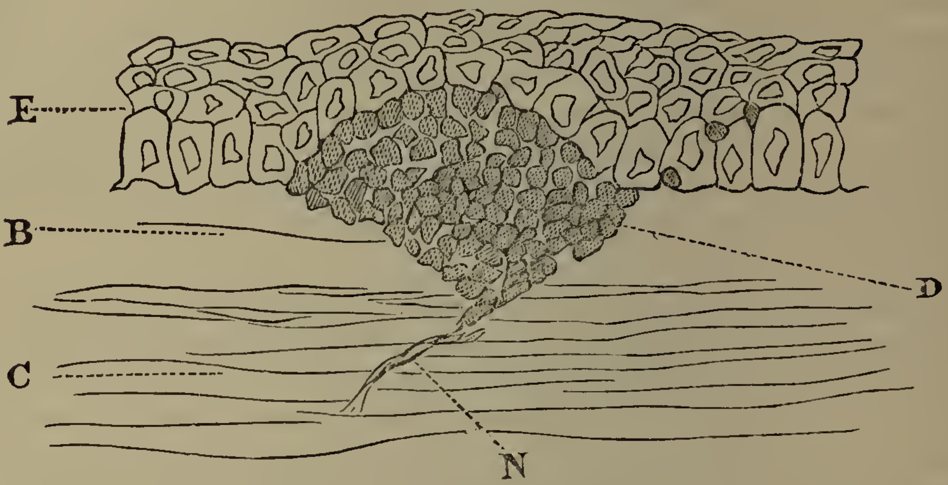
I wish now to explain to you what is known of the pathology of phlyctenular ophthalmia, before we consider its treatment. It is only lately, indeed, that anything has been made out on this point, for, as you may suppose, it is not easy to get preparations of such a disease, in order to examine it microscopically. Professor Iwanoff, of Kiew, in Russia, had the good fortune to obtain three specimens of phlyctenular ophthalmia, *post-mortem*,

FIG. I.



of which he has shown me the microscopical preparations. These demonstrate that the phlyctenulæ are collections of small, round cells, lying immediately under the epithelium of the cornea or conjunctiva. In cases of small phlyctenulæ, the epithelium covering them remains normal; in the larger ones it is usually destroyed (*i.e.*, commencing ulceration). The membrane of Bowman, on which the phlyctenulæ in the cornea lie, was always found normal.

FIG. II.—(After Iwanoff.)




E. Corneal epithelium
B. Membrane of Bowman
D. Collection of small round cells (Phylctenula)
C. True cornea
N. Nerve

You may perceive in these drawings the remarkable path by which the cells arrive between Bowman's membrane and the corneal epithelium, namely, along the course of nerve filaments, which pass from the deeper parts of the cornea, and penetrate the membrane of Bowman to reach the epithelium. You are, of course, aware that the cornea, and especially its superficial epithelium, is richly supplied with nerves. This course of the cells along the nerves is interesting, as it explains, by pressure on these filaments, the blepharospasm so frequently present, and which is often referred altogether to photophobia, attendant on the affection. If the distressing sensations of the patient were merely owing to the fact that the light hurts the eye, they ought surely to disappear when he is placed in a dark room, This is, however, not the case, for the blepharospasm is then alleviated only, but does not cease altogether. The blepharospasm must be regarded much rather as a reflex action, caused by irritation of the corneal nerves, in a manner similar to that by which an ordinary nictitation is produced. Iwanoff did not make out from whence the cells originated. In appearance they are similar to lymph corpuscles.

You will find little difficulty in the treatment of mild cases of phlyctenular ophthalmia; the instillation of a few drops of atropine, with the insufflation of a little calomel, and protection of the affected eye from exposure, will in a few days suffice for the cure. Not so, however, those cases which are attended with severe photophobia, pain, and lachrymation, all denoting extensive infiltration of the corneal tissue. If I could paint a picture of a patient with a severe attack of this kind, it would be a child of seven or eight years of age, led in by its parent, its head bowed down, and turned away from the light, the eyelids spasmodically squeezed together, the eyes covered with the hands, or the knuckles pressed into the sockets, and probably, to complete the drawing, a half-eaten piece of bread in one hand, and an apple in the other. The diagnosis of such a case is apparent, as soon as the patient comes within sight. Nothing will induce the child to give us a look at the eyes, and, in order to get it, recourse must be had to force. In this state of irritation, the only treatment admissible is an antiphlogistic one, consisting in the instillation of a dozen drops or so of atropine, (sol. sulph. atrop. gr. iv. ad ℥j.), in the course of the day, purges of calomel, and restraint in a dark room. The blepharospasm is a serious obstacle to the removal of this irritable condition, for it not only impedes the instillation of the atropine, but, in itself, encourages congestion of the eye, and thus sustains the morbid process. The children should not be permitted to lie on their faces, as they are inclined to do, for this also induces congestion. The antiphlogistic treatment, especially the atropine, and an attentive nurse, will soon succeed in abating this distressing condition of things; but still there are some cases which will resist.

In these obstinate cases, a seton in the temple is a favourite remedy with some of the London ophthalmic surgeons. For my own part I have no experience of the seton in phlyctenular ophthalmia. I can quite imagine that it is a very effectual mode of treatment, but I cannot help thinking that it is unnecessarily severe. I have had many cases of the worst phlyctenular ophthalmia under my care, but I have not yet come across one which demanded such a treatment; nor, unless indeed the phlyctenular ophthalmia which occurs in London, exceed in virulence anything which exists in Germany or here, can I believe that such cases present themselves. When the disease resists the treatment I have described to you, you will commonly find, as I have already mentioned, that the blepharospasm is the principal enemy with which you have to contend. The infiltrations in the cornea give rise to it, and it then causes congestion of the eye, which again, in its turn, encourages the formation of fresh infiltrations, and so you have a vicious circle,

in which it is necessary to break a link before you can make any progress. We have a very simple means for the correction of this excessive blepharospasm, which was always adopted by my lamented master, Von Graefe, and which I have never known to fail, if carried out properly and systematically, namely, the submersion of the patient's face in a basin of cold water. The child is raised up by two nurses, one supporting the trunk and hands, the other holding the legs, while the surgeon takes charge of the head, and dips the face under water, holding it there for about ten seconds; the patient is then allowed to take a breath, and the same manœuvre is repeated several times. The effect of the proceeding is magical: the blepharospasm disappears, the child lies on its back, allows the eyes to be easily examined, atropine to be instilled, and becomes in every way as docile as could be wished. The process may require to be repeated after some hours in consequence of a return of the spasm, but the latter will soon be permanently conquered. When, then, you have reduced the irritation of the eye, the question of a more active topical treatment arises, and there is one application which I can confidently recommend to you, as being little short of a specific in this disease, namely, Pagenstecher's yellow ointment.* About the size of a pea of this ointment is to be inserted between the eyelids with a camel-hair pencil, and after five minutes, what still remains of it in the eye is to be carefully washed out with a soft sponge. The application is to be repeated daily, and the beneficial effect of it will be soon observable—the infiltrations becoming absorbed, and the ulcers filling up. An important point in the use of this ointment is, that it be not employed until the irritative stage is quite passed. If this be not attended to, the irritation is increased, and all the symptoms aggravated. The effect of this first application should be well noted, and if the eye appear more irritated next day, the further use of the ointment should be postponed for a little longer. Calomeldusted into the eye is a favourite remedy with some; but I use it only in the after-treatment, unless in mild cases. The instillation of atropine must be continued until all irritation has disappeared. In order to prevent recurrence of the ophthalmia, treatment,

* I have only been able to have this ointment properly prepared in Dublin by Mr. C. R. C. Tichborne, of the Apothecaries' Hall of Ireland. He has given me the following note of the process:—"The precipitated mercuric oxide was made by me in the following manner,—Corrosive sublimate was dissolved in hot water, and this solution was precipitated with a solution of potash. The latter should only be added until all the oxide is precipitated. The mercuric oxide falls as a yellow powder. It must be carefully washed with distilled water, and dried at 100° C. The ointment is prepared by rubbing up one part of this precipitated oxide with fifteen of unscented cold cream. If properly prepared, this is just the right consistency. It should, however, be genuine cold cream, and not what is ordinarily sold as such, which is only an ointment. That is to say, it should contain water, and be made with unbleached almond oil."

either with the ointment or with calomel, must be persisted in long after the affection is apparently cured; for, the most common cause of these recurrences is the too early cessation of the treatment. So long as the eyes are red and injected upon the patient's awaking in the morning, the cure is incomplete. A complication which may retard the cure, and cause recurrence of the affection, would be the existence of a stricture of the nasal duct; you must, therefore, be on your guard against it.

There is no doubt that this form of ophthalmia is most frequently found in children of a strumous habit, indicated, in well-marked cases, by enlarged glands in the neck, a red and bulbous nose, and a swollen and over-hanging upper lip. I am inclined to think that in the Profession, the influence of the constitution upon local diseases in general is often over-estimated; and that the result of this is to encourage the error among us, of our consoling ourselves for our inability to cure certain diseases, because they are supposed to depend upon an abnormal condition of the constitution; whereas, in fact, perhaps, the fault lies in the unsuitability of the local treatment employed, in the mode in which it is applied, or, in some local complication, which should be corrected. I believe that the great thing for the cure of phlyctenular ophthalmia, and for the prevention of its recurrence, is the local treatment, and I do not think (I confess I have never seen it tried) that any constitutional treatment would alone effect the cure. At the same time, it is most important that you should attend to the general health of these patients. You must look after the digestive organs, and regulate the diet. Above all things, the constant chewing of apples, cakes, and bits of bread must be forbidden. It is a habit which is almost universal with these children, and one which is often encouraged by the parents, who think it a sign of robust health. The meals must be given at stated hours, and be of a plain and wholesome kind; but between times the stomach must have leisure to digest its contents and to rest, and must not be kept in a state of irritation by having fresh work thrust upon it every now and then. Cod-liver oil, internally, will suggest itself to you all as specially indicated. Von Graefe was very fond of prescribing what he called Plummer's powders, containing calomel and golden sulphuret of antimony in equal parts.

In conclusion, I must warn you against the terrible mistake, which is prevalent, that the subjects of phlyctenular ophthalmia, "grow out of it," and that it is therefore unnecessary to take any measures for its cure. And so, indeed, they do "grow out of it;" but with their corneæ in so nebulous a condition, that their future prospects are often seriously affected in consequence. —*Medical Press and Circular, Nov.* 29, 1871, *p.* 476.

## 72.—TREATMENT OF CONICAL CORNEA BY REMOVAL OF THE TOP OF THE CONE.

By C. Bader, Esq., Ophthalmic Assistant-Surgeon, Guy's Hospital.

[In 1863 the first case was operated on in this manner, and up to the present time nine cases altogether have been thus treated. The operation consists in removal of the top of the cone, and the results have been favourable.]

1. Place the patient on a bed as for extraction of cataract, and bring him thoroughly under chloroform.

2. Keep the eyelids open with a stop-speculum, so as to press upon the eyeball as little as possible, and fix the eyeball with the forceps.

3. Immediately before commencing the operation, ascertain the position of the top of the cone by turning the eye to be operated on in different directions, while strong light is thrown upon the cornea with a two-inch convex lens.

4. The subsequent steps of the operation, however they be taken, should tend to remove the top of the cone (the entire thickness of the cornea), so as to cut an opening through the cornea into the aqueous chamber. This opening, somewhat oval-shaped, is from one-twelfth to one-sixteenth of an inch in its longest diameter; it was measured immediately after removal of the apex of the cone, after escape of the aqueous humour, the cornea having collapsed and being in contact with the iris. The pupil, in all cases, was opposite the opening in the cornea; the surface of the lens could be seen bulging into the area of the pupil. In the cases in which the thread was used, the latter caused an indentation in the crystalline lens while passing across the area of the pupil.

5. *Removal of the top of the cone.*—A small curved needle, armed with fine white or black silk, or with silver wire (or a gilded, small, sharp hook), is thrust through the cornea in its horizontal diameter, close to the portion of cornea we wish to remove. The point of the needle, after piercing the cornea, is carried horizontally across the aqueous chamber to a spot opposite the point of entrance, and again thrust through the cornea close to the portion of cornea we intend to remove. The aqueous humour escapes before or after passing the point of the needle through the cornea the second time. The needle or sharp hook is left in the cornea until the top of the cone has been removed; it helps to protect and keep back the crystalline lens. The portion of cornea (the top of the cone) situated in front of the needle is then removed as best we can. This part of the operation is somewhat difficult, the cornea being transparent or nearly so, extremely thin and flaccid, and the

iris and lens being in contact with the cornea. The head of the needle is held in one hand, and with a cataract-knife, or with a sharp, narrow, lancet-shaped knife, an incision is made through the cornea (a small flap incision, as in flap extraction for cataract). Having made the incision, the needle is let go, the small flap seized with an iris-forceps, and the rest of the cornea (of the cone) removed with the knife or with scissors.

6. *Closure of the wound (opening) in the cornea.*—If a sharp hook or a needle without thread has been used for transfixing the cone, it is withdrawn after removal of the cone, and the wound left open. The eyelids of both eyes are then closed, kept bound up and cool with wet lint, and the patient kept in bed for three days; on the third day the use of the eye not operated on is permitted, while the eye operated on is kept bound up until all redness has subsided. If a needle, armed with silk thread, has been used, the needle, after removal of the cone, is drawn out gently by the second opening in the cornea; so that the thread, while passing across the aqueous chamber, the surface of the iris, pupil, and crystalline lens irritates those parts as little as possible. The opening in the cornea is closed by tying the thread, as is done when uniting the margins of a wound by a suture. The suture is tied tightly. If it should give a little before completing the knot, it does not signify. The cornea, by closing the suture, is thrown into numerous folds. This folded condition continued in an extreme degree in one case for nearly four weeks. Having united the wound, one end of the thread is cut off close, the others left about a quarter of an inch long, so as to assist when removing the suture. Both eyes are kept cool and bound up with wet lint. The patient remains in bed. The suture is removed on the appearance of slight chemosis and swelling of the eyelids. (See cases.)

7. *Removal of the suture.*—The patient being rendered insensible (by methylene), the lids are kept open without pressing upon the eyeball, the surface of the cornea is well cleared from mucus, &c., and the long end of the suture drawn away from the cornea, so as to stretch the suture, and the latter cut through and withdrawn. After this, the lids of both eyes are again carefully kept bound up with wet lint. The use of the eye not operated on is permitted on the third day after removal of the suture. The operated eye is kept bound up, and the lint cool, until all redness of the eyeball has subsided, or nearly so, when an artificial pupil is made.

8. Any other mode of destroying the apex of the cone in conical cornea—for instance, by the galvanic cautery—would, I believe, answer as well as abscission of the cone.

*Disadvantages of the Operation.*—An opaque spot in the

cornea, from where the cone had been removed. The patients were told that there would, after the operation, remain a small white speck on the eye, but that nothing except an operation could improve sight. None of the patients complained of the disfigurement, and all were pleased with the amount of vision obtained.

The necessity of giving an anæsthetic repeatedly. For removal of the suture, and for the artificial pupil, methylene is quite sufficient.

*Advantages of the Operation.*—In extreme cases of conical cornea a greater improvement of sight is obtained than by any of the known modes of treatment.

No untoward accident occurred in any of the cases.

The after-treatment, after removal of the suture, is very simple. The patient need only be seen at great intervals.

The conical cornea in all cases completely disappeared, and gave way to an abnormally flat cornea.

*Case* 1.—O. M., aged twenty-four, female, delicate, thin. Conical cornea in both eyes; cone of the left eye opaque. Iridesis had been performed in left eye in 1862, and iridectomy on both in 1864, without result. Left eye unable to recognise letters, or to guide herself. Lately attacks of ophthalmia, photophobia, increased prominence of the cone. Sept. 28th, 1864: Chloroform. Left eye—removal of the apex of the cone: closure of the wound by a silver-wire suture. Oct. 4th: Suppuration of the cornea round the wire suture; removal of the suture; the corneitis soon subsided. Nov. 5th: Both eyes kept bound up for one week, and the operated eye for six weeks. Left eye, dense central corneal opacity, of about the size of a pupil of medium dilatation; no trace of cone; iridectomy inwards and downwards. Feb. 9th, 1865: No trace of cone; tells time on watch at eight inches. August, 1871: No trace of cone; the left eye, as regards vision, is now the best of the two.

*Case* 2.—M. M., aged thirty-four, female, thin, delicate. Conical cornea in both eyes; opacity near the apex of left cone; right near-sighted; left the same, with amblyopia; recognises No. 40 of Snellen with difficulty at one inch. A large crescentic atrophy is seen in the choroid at the outer side of the left optic disc. Feb. 6th, 1868: Chloroform. Left eye—removed the apex of the cone (cornea extremely thin). 29th: Some ciliary redness, with slight intolerance of light, and watering; central corneal opacity; no trace of cone; left the hospital. April 24th: Iridectomy inwards and downwards. Oct. 3rd, 1871: Left eye—central corneal opacity; no trace of cone; cornea very flat; time on watch at 3″; 200 at 5′.

*Case* 3.—G. C., aged thirty-seven, male, thin, delicate.

Always slightly near-sighted; the last few years the left eye had been failing more. March 21st, 1870: Both corneæ conical and transparent, the left most; with left eye recognises time on watch at about one inch. April 25th: Von Graefe's operation performed on the left cornea; touching with caustic was discontinued, the patient being unable to attend regularly. May 9th, 1871: Removal of apex of cone of the left cornea; one suture inserted. July 18th: Left eye, iridectomy inwards. November 17th: Right eye, − 7; recognises No. 70 of Snellen at 5′. Left eye, + 10; recognises 50 of Snellen at 5′. For work and reading gave spectacles with — 7 for the right, and + 10 for the left, eye. In this case an extreme degree of myopia has been changed into one of hypermetropia of $\frac{1}{10}$.

*Case* 4.—Y. D., aged thirty-five, male, delicate, thin; sixteen years ago had some form of inflammation in left eye; sight of this eye failing since then. Right eye more near-sighted the last twelve months. May 18th, 1870: Left eye—cornea transparent, conical; a slight opacity is seen at the apex of the cone when examined with a convex lens. Recognises 200 (Snellen) at 1′. Right eye—slight conical cornea; 70 of Snellen at 20′. Left optic disc appears very distorted. Left eye not improved practically by calabar, atropia, or similar remedies. 24th: Cut off apex of cone (under chloroform) from left cornea; one suture inserted. 27th: Suture removed; cornea quite clear, but thrown into folds; immediately after removal of suture cornea assumed a smooth good curve, with an uneven loss of substance, resembling an ulcer, at the spot from which the cone had been removed. 29th: Slightly bulging uneven opacity and ulcer from where the cone had been removed; slight redness, no pain, and good aqueous chamber. Patient left London, keeping the operated eye bound up and using the fellow eye for work. Aug. 12th: Left eye—central corneal opacity of the size of a moderately dilated pupil; no trace of conical cornea; pupil dilates well under atropia; iridectomy inwards and downwards. Aug. 23rd, 1871: Left eye—no trace of conical cornea; small central corneal opacity; the optic disc of natural shape and well seen; reads 70 (Snellen) at 20′; does the same with a convex lens of 24 inches; the sight of this eye is still improving. In this case we see an extreme degree of myopia with amblyopia, owing to conical cornea, changed into a hypermetropia of one-twenty-fourth.

*Case* 5.—J. R., aged thirty-seven, thin, delicate, both eyes failed at the age of nineteen; does all his work with the right eye. August 4th, 1870: Right eye recognises 200 of Snellen at 20′; left eye does not recognise 200 at 1′. Both optic discs are visible, but appear very distorted; both corneæ are conical,

with a small opacity at the apex of the left cornea. June 3rd, 1871: Chloroform; removal of apex of cone of left cornea; an opening of about the size of a small pupil was thus made into the cornea; it was closed by one suture; this caused the cornea to be thrown into large folds, which diverged from the suture. Very sick all day after the operation. 8th: Much pain and swelling of eyelids; cornea clear and much folded; iris in contact with cornea; chloroform; suture removed. 26th: Slight chemosis and swelling of lids; painless; cornea clear, except where suture had been; there is an opacity; the cornea is still very much thrown into folds; iris in contact with cornea. Aug. 24th: Curve of cornea good; large central opacity; good aqueous chamber; a small piece of the suture, which had been left in the cornea, hidden by pus, was removed. 27th: Left eye—iridectomy inwards; bleeding into aqueous chamber. Oct. 27th: The optic disc, well visible through the iridectomy pupil, appears much distorted when viewed through some, regular when seen through other parts of the cornea. Patient states that never before the operation has the sight for distance been as good with the operated eye as it is now. The patient is still under observation. It will be remarked that the folded state of the cornea, and the contact of the iris with the cornea, persisted for more than six weeks.

*Case* 6.—S. P., aged sixteen, female, delicate, thin; became near-sighted five years ago, left most, iridectomy upwards performed elsewhere without result; right eye recognises No. 50 of Snellen at 70′, left eye No. 200 at 3′. April 17th, 1871: Chloroform; apex of cone removed from left cornea. 21st: Opaque matter occupies the spot where the apex of the cone had been. 28th: Much intolerance and watering; opaque matter disappeared; slightly bulging vesicle in wound; good aqueous chamber. Aug. 12th: The bulging vesicle has become opaque; left hospital. Sept. 11th: Iridectomy inwards and downwards. Oct. 2nd: Left eye recognises 200 of Snellen at 3′; with 3 convex recognises 70 of Snellen at 12′, and letters of IV. at 1′. The cornea is very flat, with a central opacity. Thus an extreme myopia has been changed into a hypermetropia of one-third. One is inclined from the above to infer a peculiar shape of the crystalline lens.

*Case* 7.—G. M., aged twenty-two, female, fair, stout; three years ago some kind of general illness (fever), with ophthalmia in both eyes; this was followed by great pain in the head and by impaired sight, especially of the left eye. Feb. 6th, 1871: Both corneæ transparent, conical, left most, with a large shadow thrown by the cone; both optic discs as to colour appear healthy; right eye with — 20 recognises 70 of Snellen at 20′; left eye does not even recognise 200 of Snellen close to

the eye. June 8th: Left eye—chloroform; removal of the apex of the cone. Oct. 8th: Iridectomy inwards and downwards. Nov. 9th: Left eye—no trace of cone; cornea very flat; central opacity; vision improving; 200 at 20′ with + 10, time on watch at 6″.

*Case* 8.—W. S., aged twenty-four, female, delicate, thin; near-sighted as long as she remembers; extreme conical cornea in both eyes; the cones transparent; tells time on watch with difficulty at 2″; is not able to work; no concave lenses are of use. Aug. 4th, 1871: Chloroform; removal of apex of right cornea; an opening the size of a pin's head cut into the cornea; *no suture;* bandage over the right eye. 28th: Left cornea operated on in the same manner; *no suture;* bandage for five weeks, then shade. Oct. 16th; Both eyes no trace of cone; small faint central corneal opacity; central round clear pupil; tells time on watch at 16 inches.—*Lancet, Jan.* 20, 1872, *p.* 73.

---

## 73.—THE TREATMENT OF CHRONIC CONJUNCTIVITIS.

Mr. Liebreich relies almost exclusively on nitrate of silver as a stimulant application in the treatment of chronic inflammatory affections of the lids and the superficial structures of the globe. The patients mostly attend daily and receive the application in the form of a solution, of the strength of ten grains to the ounce, from his own hand. All the cases of corneitis were treated either with this solution or with atropine.—*Lancet, Dec.* 2, 1871, *p.* 778.

---

## 74.—ON A NEW METHOD FOR THE EXTRACTION OF CATARACT.

By R. Liebreich, Esq., Ophthalmic Surgeon to St. Thomas' Hospital.

[The original method of flap extraction of cataract was abandoned by Von Graefe on account of the large per centage of suppurations which occurred. He came to London in 1862, and was surprised at the great success which Bowman and Critchett had achieved in the operation by the invention of better instruments (broad lancet-shaped knife, Bowman and Critchett's spoon). This caused Graefe to reconsider the subject, and he soon afterwards brought out his operation by linear extraction. This operation, although adopted by most ophthalmic surgeons, did not satisfy Mr. Liebreich, for reasons stated in this paper, and he now brings before the profession an easier and more certain method of procedure.]

For the better understanding of what follows, I deem it essential to mention here the principal points of the comparative considerations.

Let us first compare the incisions in regard to their position, form, and execution. Originally Graefe's incision was made as peripherical as possible in order to avoid the tissue of the cornea supposed to be unfavourable for union. Later he approached more to the margin of the cornea, the external surface of the wound, however, remaining always above and beyond it. By its linear form, as nearly straight as it can be on the surface of a globe, it contrasts most markedly with the flap incision. The latter almost semicircular, situated everywhere within the cornea itself (not to speak of a modern modification), does, indeed, offer quite different conditions for the succeeding steps of the operation. Whilst the linear wound remains closed even in excentric movements of the eye, and can only be made to gape by pressure, the flap wound has so great a tendency to open, that a brisk movement of the eye, or a slight pressure with the finger on the edges of the wound causes them to be widely separated. On this point chiefly rests the difference in the mechanism of the removal of the lens. In the flap extraction, the capsule being opened and the wound made to gape, the lens rotates so that the point of its equator most distant from the centre of the wound serves as centre of rotation, whilst the margin of the lens towards the wound moves forward. In this movement the lens passes over the posterior surface of the iris, like the fœtal head over the perinæum, and protrudes the iris, in order to place itself in the wound, having first overcome the resistance of the sphincter pupillæ. This mechanism cannot take place in Graefe's incision. If after his incision the capsule be opened and the wound made to gape, and the lens to advance further forwards, it would press against the whole posterior surface of the iris, and could never pass the pupil; it is, therefore, in this method, a mechanical necessity to cut out that part of the iris directed towards the wound up to the sphincter, or, as Taylor proposed, to incise it at the periphery. But even after excision of the iris, after the opening of the capsule, the making the wound gape cannot force out the lens; consequently, traction instruments (such as Bowman and Critchett's spoon, Graefe's crotchet), were originally used. Later, the introduction of these instruments was replaced by external manipulations. With a spoon of caoutchouc (Graefe) or tortoiseshell (Soelberg Wells), the wound was made to gape and the lens thrust upwards. The latter is done by pressing from below upwards upon the cornea.

In the greater tendency of the flap of the cornea to gape rests also (and as it appears to me exclusively) the difference in the

healing, in the necessary precautions and in the results which are to be obtained by each of these two methods.

That the more favorable results of Graefe's operation are not due to a greater tendency of the tissue of the sclerotic to heal than that of the cornea (as has been supposed for a long time), there can now be no doubt, since it has been shown that even in these incisions, made according to the original plan, the superficial part only belongs to the sclerotic, the deeper must always be in the cornea. In regard to the peripheral position of Graefe's incision I am fully persuaded that it not only does not impart to his operation any specially favorable influence, but that, on the contrary, it is the principal objection to it. I shall have occasion to recur to this point.

Thus the linear form of the wound and its lesser tendency to gape alone remain the cardinal points, to which we owe, in Graefe's method, the lesser percentage of total suppuration than in flap extraction, the diminished necessity of precautions and restrictions for the patients; also that, even in cases of very bad general constitution, weak and decrepid individuals with thin and flabby cornea, the prognosis is not so unfavourable as in flap extraction.

Even the latter fact does not appear to me to be due to the unfavourable nutrition of the flap, but rather to the more mechanical condition that the linear wound of the cornea, even where from want of elasticity it folds itself up and sinks in, immediately closes, so soon as the aqueous humour begins to re-accumulate, which is not always the case in flap wounds.

No wonder that in consequence of these advantages of Graefe's method the flap extraction became soon more and more abandoned. To me, however, it appeared that the mechanism of Graefe's method was still too complicated, and too violent, that prolapse of the vitreous body, and hemorrhage into the anterior chamber, were too frequent during the operation, iritis, and strangulation of the iris in the corners of the wound too frequent after it, and that the most favourable results, compared with the most favourable results in flap extraction, were not perfect enough. If these inconveniences are carefully inquired into, it is found that they can all be brought back to one, and the same principal cause; namely, the peripheric position of the incision. This peripheric position explains why:

1. It is impossible to remove the lens without iridectomy.
2. The excision of the iris must be very extensive, else the iris tends to prolapse.
3. It is necessary to perform the operation above in order that the enlarged part of the pupil may be covered by the upper eyelid; the removal of the lens upwards is far more difficult on account of the tendency of the eye to roll upwards; and consequently,

4. During the whole operation the eye has to be kept open by the speculum, and to be drawn downwards by the forceps. This is not only painful and injurious to the eye itself, but causes

5. Not unfrequently prolapse of the vitreous body to which a peripheral incision itself already tends. Prolapse of the vitreous body, and hemorrhage into the anterior chamber, are the chief impediments to a careful removal of all the *debris* of the cortex and cause

6. Those grave forms of iritis which are kept up by the permanent irritation caused by the tumefied remains of the lens behind the iris.

Of these disadvantages I was perfectly aware, after I had followed for a short time Graefe's original plan, and I proposed some modifications, therefore, in 1867, in an article on cataract which I wrote for the 'Nouveau Dictionnaire de Médecine et de Chirurgie,' Paris, Baillière. They, however, were but a first step, and during the last four years I have by a large series of systematic experiments arrived at a method, which now, after more than 300 operations performed by it, I consider definitely settled.

Gradually I removed my incision more and more from beyond the margin towards the centre of the cornea, so that it is now entirely situated within the cornea, with the exception of the points of puncture and contra-puncture, which are placed about one millimeter beyond it in the sclerotic*—the whole remaining incision passing with a very slight curve through the cornea, so that the centre of it is about 1½—2 millimetres within the margin of the cornea, and consequently nearly in that place towards which the equator of the lens is turned, when by a slight pressure on the margin of the wound the lens is brought into that rotation of which I have spoken above in flap extraction. In this form and position of the wound a broad excision of the iris could be avoided, and consequently I cut off smaller and smaller pieces of the iris, until at last I entirely gave up iridectomy. I now saw that without actual formation of a flap that mechanism can be brought about, by means of which the advancing equator of the lens overcomes the obstacles of the iris and of the sphincter pupillæ in order to enter the wound. Avoiding iridectomy, I could do without elevators and forceps, and thus change the whole operation into a less violent and almost painless one. Made with Graefe's knife, the most perfect regularity can be very easily obtained without fixation. My operation is now made in the following way:

* Of course only as regards the outside of the wound. As regards the inside, all the wound, even the puncture, is situated in the cornea, the peripheral part of which cannot be reached by a knife introduced in the indicated position without previously passing through a small portion of the sclerotic.

The patient lies on his back; chloroform is administered only on his express desire; the pupil having been dilated with atropine the day previously, if possible. For the right eye the operator stands behind the head of the patient, for the left at his left side. An assistant is not necessary. All the instruments required are two, namely, the smallest Graefe's knife possible, and a cystotome which has a common Daviel's spoon at the other end. The whole may even be united in one instrument, which has at one end Graefe's knife, and at the other end Daviel's spoon, and within the handle a cystotome which admits of being pushed in and out. Supposing the right eye is to be operated upon. In that case the operator takes hold of the upper eyelid with the index finger of his left hand, whilst he slightly presses the middle finger against the inner canthus of the eye. The knife held in the right hand with its back horizontal and backwards, the plane of the blade making with the horizontal meridian of the eye an angle of about 45°, enters the sclerotic with its point about one millimetre externally to the margin of the cornea. *Without altering the direction*, the knife passes through the anterior chamber in order to make the contra-puncture on the opposite side, so that the point of the knife becomes visible in the sclerotic about one millimeter (or less) distant from the cornea. The knife is now pushed forwards, so that its retraction finishes the incision. As soon as the incision is made, the eyelid is to be dropped.

The second part of the operation consists in the careful opening of the capsule.

In the third part Daviel's spoon is slightly pressed against the inferior margin of the cornea, and the index finger of the left hand, which holds the upper eyelid, through it exerts a very slight pressure on the highest point of the cornea. Thus the lens is made to rotate a little, its lower margin presses in the manner already described against the posterior surface of the iris, pushes the iris forward, passes along it to the margin of the pupil, overcomes the obstacle and places itself freely in the wound, which is made to gape by Daviel's spoon pressing against it. A slight pressing movement of the index finger of the left hand, by means of which the upper eyelid is shifted from above downwards over the cornea, serves to expel the lens. Similar movements of the lids are employed for the purpose of forcing out any remaining *dèbris* of the cortical substance, after pushing them from behind the iris towards the pupil, by gently rubbing the shut eyelids. Should the pupil then not appear round, but its margin drawn towards the wound, it regains its normal position by an outward shifting of the lower lid; or, if that be not sufficient by the introduction of Daviel's spoon. Immediately afterwards I put some atropine into the eye, and close it by my compressive bandage.

What are the advantages of my method of operating?

1. It is undoubtedly of all methods the simplest and the least painful.

2. It is unconditionally the easiest to perform, and requires the least practice; it may, therefore, be performed by those who have only occasional opportunities of operating; and those patients will profit by it who are unable to reach a central point in order to place themselves in more practised hands. On account of the greater facility of operating, the last pretext for reclination of cataract is removed, which, though almost universally and justly condemned, is still here and there performed.

3. It is preferable to the flap extraction, on account of the safer and constantly regular incision. The flap incision scarcely ever acquires the regularity which may theoretically be demanded, even if made by the most practised operator, with the best assistance, the most enduring patient, or under chloroform by the use of the elevator and fixation instruments. At one time its height or breadth is not what it is intended to be; at another time its position is incorrect or the wound irregular. Indeed, part of the irregularity is due to the difficult form of the incision, but by far the greater part, according to my conviction, is due to the mechanism by which the cuneiform cataract knife makes the incision. A small Graefe's knife would make a flap safer and more regularly than any wedge-shaped cataract knife. The incision I designed can easily be made, in every case, exactly in the desired form and position, even if the patient is very restless, without assistance, without elevator or fixation. This mainly depends on the facility with which the place of the contrapuncture can be chosen, the knife drawn back and made to pierce at another point, if a mistake is made in the selection of the place for contrapuncture, and in the freedom with which in terminating the incision the inclination of the knife can be changed, if necessary.

A little practice will enable every operater to avoid those corrections, and to make the contrapuncture, as well as the whole incision, correctly, according to his original plan.

4. Compared with Graefe's method, it has the advantage of a more favourable position of the field for the operation, and avoids through it all the inconveniences to which I have referred as arising out of the peripheral position of the wound.

5. In regard to the mode of healing, it favourably contrasts, like Graefe's method, with the flap extraction, on account of the diminished influences which age, constitution, general state of health, season, and other causes exert: also on account of the less demand made upon the patient to remain quiet after the operation, and, above all, on account of the lesser tendency to suppuration of the cornea.

6. The advantages of my method over that of Graefe are shown by the ultimate results obtained—by not furnishing a greater percentage of total suppuration than Graefe's method my best results are in regard to optical and (if I may use the term) anatomical perfection, identical with the best results obtained in flap extraction.

The method is well adapted for the different cataracts, with the exception of—

1. Those laminellar cataracts, which need only be treated by iridectomy.
2. Cataracts which in earliest childhood have to be operated upon by repeated division.
3. Perfectly liquid cataracts, (division with a broad needle).
4. Partial cataracts, without a nucleus, already absorbed to a great extent, and therefore chiefly traumatic cataracts, for which also division suffices.

In all other cases of cataract my proceeding can be adopted, only the breadth of the incision and its obliquity may vary very slightly, according to the size of the nucleus, the consistency of the cortex, and especially to the proportion which the size of the cataract bears to that of the cornea. In cases of equally softened cortex, with a smaller nucleus and sufficiently large cornea, puncture and contra-puncture may be made low down and closer to the margin of the cornea, and thus by slighter inclination of the blade, the incision obtains a strictly linear form. With a large and hard nucleus and tough immature cortex it will be necessary, especially if the cornea is exceptionally small, to make the wound somewhat larger and a little more curved. For that purpose puncture and contra-puncture must be made a little closer to the horizontal meridian of the eye, and the blade somewhat more inclined (a little more than 45° from the horizontal meridian).

In this way it is in every case possible to make the wound sufficiently large for the easy passage of the most voluminous cataract, without incurring the tendency to gape. Even for the removal of the lens with the capsule (without opening the latter), the wound is sufficient.

If Pagenstecher's procedure is followed, my incision must then be combined with iridectomy. Delgado's procedure can be followed out, even without iridectomy. Delgado introduces a small instrument into the anterior chamber, before he makes his incision, and presses with it on the lens, in order to free it from its natural connections and to remove it, together with the capsule, after the incision has been made.

I intend to propose another method of extracting the lens together with its capsule; but a more extended series of experiments is required before I can definitely decide the question,

whether and in what cases this procedure can be usefully combined with my incision.

The question, if and in what cases my procedure is to be combined with iridectomy of more or less extent, can be solved only after a larger experience of my operation without iridectomy than I yet possess. I have performed several hundred operations with iridectomy, but as yet scarcely 100 without.

I hope that I shall not be left alone to work out this question, but that other surgeons will accept my proposal, which, to recapitulate shortly, is as follows :—

To avoid the disadvantages in Graefe's operation arising out of the peripheral position of the wound, and the disadvantages in flap extraction arising out of the height of the flap, I propose a new method of extraction, which is to be made in the following manner :

Puncture and contra-puncture are to be made in the sclerotic about 1 mm. beyond the cornea; the whole remaining incision is to pass, with a very slight curve, through the cornea, so that the centre of it is about 2 mm. distant from its margin. This incision may be made upwards or downwards, with or without iridectomy, and the lens may be removed through it, with or without its capsule.

If, as I now operate, the extraction is made downwards without iridectomy, the whole proceeding is reduced to the greatest simplicity, and does not require assistance, elevator, fixation, or narcotism, and only two instruments.

In this latter form my procedure has the advantage of being on the one hand, the least dangerous mode of extraction; and on the other, of being able to obtain the most perfect results, viz. that condition in which the eye operated upon differs from the normal eye only by the absence of the lens, and a cicatrix of the cornea difficult to be seen.—*St. Thomas' Hospital Reports*, 1871, *p*. 259.

---

## 75.—ON EXTRACTION OF CATARACT BY A PERIPHERAL SECTION OF THE IRIS WITHOUT INJURING THE PUPIL.

By Dr. Charles Bell Taylor, Surgeon to the Nottingham and Midland Eye Infirmary.

[Dr. Taylor has found the operation here described very successful, not more than four or five unfavourable cases having occurred to the hundred. The advantage claimed for it is a central and moveable pupil.]

Prolapse of the iris has always been the bugbear of extraction wherever iridectomy has not formed a part of that operation; and yet the extreme beauty and superexcellence of the

results, when no accident has occurred and when all has gone well, after one of the old flap operations, is such that we constantly find ophthalmic surgeons abandoning the modern methods to revert to the old and necessarily much more dangerous flap extraction. When I was last in Utrecht I found that Professor Snellen had, in spite of the good results to be obtained by Von Graefe's and other modern methods, returned to the old flap operation; and the same is true of many other continental surgeons as well as of some of the most eminent in this country. Those, for instance, who have had the good fortune to study Mr. Bader's practice at Guy's Hospital must have noticed how frequently and with what excellent results he resorts to the old flap operation. I have myself on several occasions, actuated by a similar desire to save the iris and attain the maximum of success, extracted by my own method without excising any portion of the iris, merely enlarging the wound in a lateral direction sufficiently to permit of the exit of the lens. Dr. Macnamara, of the Calcutta Hospital, has also endeavoured to effect the same object by the use of the spoon. In my own experience, however, prolapse has occurred from time to time, and whenever I have left the iris untouched I could by no means say, as I was in the constant habit of doing when every step of my process had been fully carried out, that my anxiety had ceased with the operation, and that scarcely any subsequent treatment was necessary.

Latterly I have been endeavouring to ascertain whether it were not possible to combine the prevention of a prolapse and the other advantages attendant upon an associated iridectomy with the central and movable pupil so much to be desired in all operations of extraction for cataract, and having attained that desirable consummation, I am anxious to lay before the profession the steps of the process, which are briefly as follows.

As a rule I administer a mild aperient the day before the operation, and extract the following morning before breakfast. Chloroform is much more pleasantly given under these circumstances. The risk of vomiting is to a great extent obviated, and that of all other accidents dependent upon anæsthesia diminished.

The instruments I employ are a pair of sharp forceps that pierce the sclerotic; a very light speculum (a modification of Von Graefe's); and two knives, a line in width, and bent at an angle similar to the ordinary iridectomy knife—one with a sharp point, the other with a blunt or bulbous extremity.

Having separated the lids with the speculum, the eye should be gently turned downwards with a pair of ordinary forceps in the operator's right hand. Having got the globe into a favourable position, it should be fixed by the sharp forceps at about

the junction of the upper with the middle third of the cornea; the pointed knife is then entered in the cornea-sclerotic junction one or two lines from the forceps at the summit of the cornea, pushed well into the anterior chamber, and then, with a gentle sawing motion, carried along the summit until about one-third of the cornea has been incised. The capsule is then carefully divided with Von Graefe's cystotome, having been previously rendered tense, and the eyeball fixed with a pair of ordinary forceps. (It is better to open the capsule at this stage, because bleeding from the wounded iris—and conjunctiva also—at a later period is apt to fill the chamber and render this part of the operation obscure and difficult.) The upper segment of the iris is then seized, and a small piece of the periphery only excised, the pupillary margin and portion of iris attached to it being left untouched and free in the anterior chamber; the lens is then extruded through the gap in the ordinary way, gliding behind the pupil, so that there is no stretching of the sphincter.

In this way I believe that I have secured all the advantages, in the way of safety and certainty, of an associated iridectomy (which I have already detailed), and at the same time attained that great desideratum—a central and movable pupil.—*Lancet, Nov.* 4, 1871, *p.* 634.

---

## 76.—ON CATARACT.

By Dr. H. R. SWANZEY, Ophthalmic Surgeon to the Adelaide Hospital, Dublin.

The cataract of old age or senile cataract, is the most common form of this disease. It usually attacks persons over fifty years of age. In it the central portion, or nucleus of the crystalline lens is the most opaque and hardest part, while the cortical substance is much more translucent, even when the cataract has become quite ripe. This depends upon the physiological changes which take place in every crystalline lens with advancing years. In the youthful crystalline the central part is not more consistent than the cortical layers; but, before middle life is attained, a distinct nucleus of a firmer texture is found, which becomes more marked at later periods. In old age, too, the lens, and especially its nucleus, obtains an amber tint, which is sometimes of so deep a shade that you must be careful not to mistake it for a cataract. Senile cataract, indeed, appears to be one of those conditions which stand on the boundary of what is pathological and what is physiological. We may yet, no doubt, have much to learn as to the cause of the affection, but, so far, we know that cataract frequently makes its appearance in persons otherwise perfectly hale, and,

therefore, when you find it in some who have suffered much from gout or rheumatism, it would be irrational to seek its cause in the latter or any other concomitant constitutional disease. Nor has the microscope been as yet successful in demonstrating changes in the choroid or vitreous humour, which would account for the alterations in the lens on the supposition of a locally impaired nutrition. We may, therefore, be not far astray in regarding senile cataract as an exaggerated physiological condition, a consideration which does not take it out of the category of diseases, interfering as it does with the functions of the organ. Similarly, a certain amount of chronic enlargement of the prostate gland is extremely common in old age, but is not regarded by surgeons as a disease until it attains such a degree as to impede the functions of neighbouring parts.

Senile cataract may commence in the nucleus, or in the cortical portion of the lens, or simultaneously in both. These forms, however, which commence with fine stripes in the edge of the lens, while the nucleus remains clear, are usually very slow in their progress, so much so, indeed, that if your patient be of an advanced age, it may be more judicious to refrain from mention of this condition of the eye, for he will be very apt to outlive this cataract. Broad stripes in the corticalis with accompanying opacities of the nucleus indicate a rapidly advancing cataract. In every elderly person who comes to you complaining of his vision, you must carefully examine the crystalline lens. You must do so by daylight, by the reflected light of a lamp with the mirror of your ophthalmoscope, and by means of the oblique illumination. According as the opacities in question, if they be present, are more or less intense, you will discover them best by one or other method. In using the reflected light to discover incipient cataract, it is a mistake to have it too brilliant, for then the more translucent opacities may escape detection. To examine the periphery of the lens which is concealed by the iris, you must direct the patient to look strongly to one side, or upwards or downwards, while you look into the eye from the opposite direction. When a senile cataract is quite ripe, the entire of the cortical portion has become opaque, and with the oblique light a yellow reflector is obtained out of the centre of the lens from the hard nucleus.

This is the most favourable form of cataract for operation, fully ninety-five per cent. of such eyes being restored to vision by means of Graefe's peripheral linear extraction. By means of this method, which is you see practised here, the most infirm person may be operated on. In the days of the flap extraction many bodily infirmities afforded contra-indications to its performance, as it entailed confinement to bed in a horizontal

position for a lengthened period. If the condition of the patient renders it impossible or undesirable that he should be confined to bed, he need barely lie down for more than a few hours after the extraction by Graefe's method; and in no case which runs a favourable course, need the patient remain in bed for more than three or four days. I do not now intend to take up time by a description of the various steps of the operation which you have seen performed, and which you will have another opportunity of witnessing in this case. To have a successful result, it is quite as necessary, in my mind, that the after treatment of the case be properly conducted, as that the operation itself be correctly performed. Graefe not only developed this method of extraction, but also the treatment best adapted to it; a circumstance which is very generally ignored.

Immediately after the operation the eye is carefully padded with fine charpie or French wadding, in such a way that no part of the globe is pressed on more than another; then, three turns of a narrow flannel roller are made with a gentle, but firm and equal pressure over the eye and round the head, in the manner with which you are acquainted. The patient is then comfortably settled in bed, and the room darkened. The pain of the wound should not be very severe, and should not continue longer than four or five hours; in either event you should administer about the eighth of a grain of morphine hypodermically in the corresponding temple. Pain, in all affections of the eye, is a symptom which, as a general rule, you must combat, for in itself it gives rise to irritation. In eight hours, or thereabouts, after the operation the bandage should be changed, with the object of removing the slight secretions, blood clots, &c., which are apt to collect between the edges of the lids, and which, if left there might, by their decomposition, afford the starting point for suppuration of the wound. It is not advisable, however, to open the eye or expose the wound at this early period. Between the twelfth and twenty-fourth hours is the period when you must be on the look out for anomalies in the process of healing. During this period when the course is normal, no pain or disagreeable sensation should be experienced by the patient, or at most, only an occasional and momentary "pricking." Any continued pain at this time, no matter how slight, must be regarded as a bad symptom, and be at once attended to. The bandage must be removed, any collection of tears under the lids giving egress to, and if there be any swelling of the lids as counter-irritation, they should be lightly touched externally with mitigated lapis which is then immediately neutralised with a solution of salt. If the pain still continue, a hypodermic injection of morphine should be given, and if this does not attain the desired end, then

in case your patient's strength admit of it, you should employ a venesection from the arm to the amount of four or five ounces, this should be followed by a calomel purge, and—if the interval since the previous one is long enough — another morphine injection, a very firm and exact bandage being applied. When the patient is not healthy enough to admit of a venesection, you must confine yourselves to the other means I have mentioned. With this aid you will, in a vast majority of cases, succeed in cutting short the suppurative process, which is just about being set up in the wound. But I must guard you against the fallacy of supposing that some delay in their employment is of no consequence. If you are to effect anything you must resort to them early, before suppuration has set in, for once it has done so your skill will be of little avail. You will not find that this treatment is one much adopted by ophthalmic surgeons; but it was laid down upon the basis of a most extensive field of practice by Graefe, who was endowed with remarkable powers of clinical observation, and I can testify to its value myself. That which most surgeons would be likely to take exception at, is the changing of the bandage so soon after the operation, and yet Graefe considered this one of the most important points. After the flap operation the bandage used not to be altered for some days. The venesection, too, is likely to be regarded with disfavour by many, in consequence of its being now so out of fashion. In this particular instance, I have no doubt of its frequent efficacy; and further, I believe you will see a reaction in favour of venesection in general medicine and surgery before many years, but with more restricted and precise indications than formerly. Among the many advantages afforded by the peripheral linear extraction, is the short period during which the surgeon has to be on his guard against suppuration of the wound; from the twelfth to the twenty-fourth or thirtieth hour being the time when it occurs, and never any later. With the flap operation, the eye could not be pronounced safe from suppurative reaction for several days.

Previous to extracting a cataract, you should endeavour to ascertain whether the condition of the retina, &c., is normal; in senile cataract it will generally be found so. Complications, however, which are more common in these cases are conjunctivitis, and obstructions in the lachrymal canals and nasal ducts. These you should in every case look for, and, if present, you must endeavour to restore the normal condition, or something approaching it, before extracting the cataract; otherwise, the muco-purulent secretion collecting under the bandage is very apt to produce serious anomalies in the healing process.

I have told you that old age is the most frequent cause of cataract. The disease, however, is by no means rare at other periods of life. You will often find persons of thirty-five and forty afflicted with it, and you will not seldom observe that its occurrence at this age depends upon an hereditary tendency, some of the patients' forefathers having some fluid from the same cause, although perhaps later in life. The cataract then is generally of a softer consistence, with a small nucleus. Its removal cannot be effected with as good a prognosis as the true senile cataract.

In diabetes, as I suppose most of you know, the lens often becomes cataractous. There is nothing in the appearance of a cataract dependent upon diabetes to distinguish it from any other, and it is consequently a good general rule to examine the urine of every cataract patient for sugar. Diabetes cataract may be removed with success, and it will always be your duty to remove it unless a fatal termination to the general disease appear imminent. We know how long patients sometimes linger under this disease, and it surely would be inhuman not to afford them the boon of vision for the last few months of their existence, when it may be given them with so little pain. My own experience of extraction in diabetes cataract is not sufficient to form from it an estimate as to the percentage of successful cases. It happens that I have not yet seen an unsuccessful one. Graefe found that in twenty cases of cataracta diabetica, sixteen recovered their vision by extraction.

Cataract may be produced traumatically; the most common injury being that caused by the entry of a foreign body into the eye through the cornea, and then into the lens itself. When the capsule of the lens is opened, the action of the aqueous humour on the fibres of the lens is sufficient to make them swell and become opaque. The foreign body may have been a large one, which was again withdrawn from the eye, or it may have been a small atom of metal which continues in the eye. These are often most delicate points to decide, and they are of the utmost importance, as, in case of a foreign body lodging in the eye, the ultimate prognosis for the organ is very serious indeed. You will arrive at a conclusion by the history of the case and by an examination of the eye itself. Besides penetrating wounds, blows on the eye are sufficient to cause cataract, whether given by the fist or by the cork of a soda water or champagne bottle, or by the end of a billiard cue, the patient standing behind the player while he is making his stroke. It is probable that in these cases the capsule of the lens is ruptured in some place by the concussion, as otherwise it would be difficult to understand how such injuries could cause opacities of the lens.

Cataract may depend upon deep-seated disease in the eye, *e.g.*, detachment of the retina, certain forms of choroiditis, retinitis, and irido-choroiditis. In most of these cases the external appearance of the eye gives no clue to the condition of things lying behind the cataract. The ophthalmoscope of course is of no use, in consequence of the opacity of the lens; and it is by the state of the functions alone that we can determine whether the cataract be complicated or not. If you hold a muffed milk glass close before your eye you will be able to realise the disturbance to vision caused by a fine uncomplicated cataract, and at from one to two feet you will be able to distinguish the light of a gas flame turned down to the lowest, and to see the motion of the hand in a favourable light at a foot or so from the eye; in a dark room you will also be able to tell from what direction the light from the lamp comes; this latter we term the "power of projection." If in any of these particulars a cataractous eye be not up to the mark, you must be very suspicious of the presence of complications, especially so if the projection be incorrect or defective, for this shows that that part of the retina upon which the light falls is probably diseased. Under such circumstances you would either not extract the cataract at all, or, if you thought the disease in the fundus oculi to be of limited extent, you might do it, having prepared your patient for an imperfect result.

The patient before you is aged eight-and-twenty. In the right eye there is a cataract which is shrivelled with a thickened anterior capsule. The power of perception of light in this eye is very insufficient, and the power of projection is wanting in every direction, there can be no doubt, therefore, that there are extensive complications in the fundus, of what nature I am not prepared to say. There is the history of an injury to the eye some years ago, and, very possibly, both the cataract and the deeper complications depend upon it; indeed, if it were not for the condition of the other eye, I should almost certainly be of that opinion. In the left eye she has a mature soft cataract of some months' standing, and, as well as we can make out by a functional examination, the fundus is normal. This is very remarkable in so young a subject, and is rendered still more so if the other eye be taken into consideration. The urine is normal, and there is no hereditary tendency. It is therefore indicated to operate upon the left eye, but not on the right.

Before I conclude I may mention two other forms of cataract which you will meet with—namely, lamellar or zonular, and congenital. The first affects only certain layers of the lens, the others being left quite intact. The layers affected are generally those around the nucleus, so that the latter and the

cortical substance remain clear. If the opacity be not very extensive the patient may have very fair vision still, particularly with his back to the window, or when he shades his eyes with his hand, so that, the pupil dilating, he sees through the clear edge of the lens. This too suggests one mode of improving the vision of these persons, namely, by forming an artificial pupil in the nasal portion of the iris, through which the patient may now see in any light. But when the opacity is extensive an artificial pupil would do no good, and then you will generally find the best proceeding to be that called division or breaking up of the cataract. A fine needle is pressed through the cornea into the capsule, and the latter torn to a slight extent; the anterior layer of the lens is broken up a little with the needle. In this way the aqueous humour is admitted to the substance of the lens, which gradually softens and becomes absorbed. This operation requires to be repeated frequently before the entire lens disappears, and the entire process, which occupied several months, is sometimes attended with dangers of which I shall speak to you when next I have a case of the kind under treatment. Zonular cataract most commonly appears after the first dentition, and it is a curious fact that the subjects of it have usually suffered from convulsions during teething. The affection may also be congenital.

Congenital cataract is not uncommon. It is a soft cortical cataract, and it is doubtful to what circumstances it owes its formation. Generally speaking these are favourable cases for operation by division, provided the patient be not too old, but by the sixth or seventh year the retina, from never having been used, becomes rather inert. It is advisable therefore to operate early, if possible before the first dentition. Much mischief is done by postponing the operation until the tenth or even fifth year. It is a remarkable fact, showing how necessary practice is for the development of the functions of a part, that if at ten years of age a cataract is produced by trauma, you may obtain very good vision for the patient if you operate even forty years afterwards, but in congenital cataract the result will be but very wretched in the fifth year after birth.

This is a short sketch of some of the principal points in connection with the subject of cataract.—*Medical Press and Circular, Jan.* 17, 1872, *p.* 49.

---

## 77.—ON GLAUCOMA, PRIMARY AND SECONDARY.

By T. Shadford Walker, Esq., Liverpool.

Previously to the invention of the ophthalmoscope by Helmholtz, in 1851, the disease now known as glaucoma was only

recognised as such when it had arrived at its last, and most complete stage, that namely, in which vision was so far affected that objects could only be seen dimly in one position of the eye —the general field of vision having become greatly contracted. Nothing beyond the perception of light still remained, the pupil was seen dilated and fixed, the iris reduced to a mere ring, its colour altered, and its fibres muddy and indistinct, the lens semi-opaque, and the area of the pupil showing a greenish reflection (from which the complaint obtained its name), and the eyeball itself, when touched, being of a stony hardness. Several observers, especially Weller, Lawrence and MacKenzie, had noted and pointed out the importance of the increased tension of the globe; but the early stages of glaucoma were confounded with the symptoms of amaurosis, and passed under that name. Soon, however, after the ophthalmoscope allowed the interior of the eye to be examined, Prof. Jaeger, of Vienna, observed, and carefully described, the appearance of the optic disc in glaucoma, drawing special attention to the typical excavation, which, from the peculiar shading of the cup, was supposed to be an elevation instead of a depression. Descriptions of the ophthalmoscopic appearances, presented by isolated cases, were also published by other observers, and the increased tension of the eyeball in glaucoma was remarked upon; but it was not until the late Prof. Von Graefe, of Berlin, combining the observations of others with his own, demonstrated the connection between increased globe tension and the excavation of the optic disc (the true nature of which he was the first to discover), and the consequent pulsation of the central artery, that the real pathology of glaucoma was made clear. Von Graefe, moreover, reasoning from the success he had obtained by performing iridectomy in ulceration and progressive staphyloma of the cornea (which he explained by the relief of the tension undoubtedly produced in these cases) to the great probability of a similar result in glaucoma, at length, in 1856, had the satisfaction of announcing the complete success following iridectomy in all those cases of glaucoma where delay in resorting to it had not occasioned such alterations in the structure and nutrition of the retina and optic nerve as to render operative interference of any kind of no avail. He had thus the double glory of being the first adequately to recognise the essential character of a very serious disease, and to point out a most valuable remedy, the simplicity and good effect of which have been acknowledged by the foremost oculists of all nations.

Mr. Walker having remarked that glaucoma was chiefly a disease of advanced life, and that it resembled gout, phthisis, and other constitutional disorders, in the tendencies common to them of transmission from parent to offspring, gave a careful

description of acute inflammatory glaucoma. A premonitory stage was recognised, in which very mild and transitory attacks passed off without apparently doing the eye much damage, but eventually, these became very frequent, leaving intervals of only a few days, the sufferer began to pass restless nights, and found no relief in his symptoms on awakening in the morning; his sight became affected, and contraction of the field of vision was observed. Then, usually at night and accompanied by violent neuralgic pain in the forehead, the attack suddenly came on which led to the title given to this form of glaucoma of acute inflammatory. Rapidly increasing dimness of vision, marked dilatation of the pupil, a narrowed muddy condition of the iris, more or less turbidity of the humours, and great increase in the tension of the eyeball soon follow, and are accompanied by great constitutional disturbance. One form, coming on suddenly, and actually destroying sight within a few hours, was noticed by Von Graefe, and termed by him "Glaucoma fulminaris." Fortunately, it is of extremely rare occurrence. A case of this kind occurring in a working man who came to the Eye and Ear Infirmary, was described by Mr. Walker. This patient stated that two years previously, at bedtime, without any warning, the left eye was attacked; this was ushered in by excruciating pain, and in the morning vision was gone, not even a bright light held close to the eye being seen. In a similar manner, six weeks ago, the sight of the right eye had entirely disappeared within two days. No operation had been performed, and although he still suffered from attacks of dreadful pain in the eyes, he would not allow any operation to be performed.

The second variety of primary glaucoma, known as chronic inflammatory glaucoma, was next described. It is distinguished from its predecessor mainly by having, instead of intermissions, remissions and exacerbations; rather than by the occurrence of fresh symptoms on a previously quiescent condition. Again, the cornea is much more affected in this variety; after a time it becomes flattened, and loses its sensibility to a remarkable extent, so that in many instances it may be touched or rubbed without producing any signs of distress.

A third variety exists under the name of glaucoma simplex, and is really only a very insidious form of the last; in which the inflammatory symptoms are masked, or are so slight and transitory, that the patient does not notice them. So quietly does the disease advance, that the sight of the one eye may be completely lost, while that of the other eye begins to fail in a similar manner, and, what was ascribed to a natural failure of sight, due to increasing years, is discovered to be disease requiring prompt measures to arrest its progress.

Having completed his sketch of the three varieties of primary glaucoma, the author made a few remarks on secondary glaucoma, which, as its names implies, consists of the grafting on a previous disease of the glaucomatous condition. Speaking generally, it may be said that those inflammatory diseases of the eye which, in their course, occasion an increase in the tension of the globe, and which, on subsiding, leave behind deposits of organised lymph, binding down and agglutinating the tissues, are very apt to occasion glaucoma.

In examining these classes of cases three modes of investigation must be pursued :—(1.) A careful enquiry is made into the history of the disease, and into the patient's symptoms. (2.) An ophthalmoscopic examination is made, and this, in a typical case reveals the optic disc of a muddy, reddened, or yellowish tint, instead of showing a flat surface, being deeply excavated or cupped, the arteries smaller than natural, and exhibiting, either spontaneously or on very slight pressure, a distinctly visible pulsation, the retinal veins swollen or knotted, and very tortuous, the retina itself deprived of its transparency, becoming blurred, indistinct, and muddy, or being atrophied, and showing the choroidal pigment through its coats. (3.) Examination by the finger to determine the tension of the globe. In connection with this subject, the new ophthalmotonometer, invented by Professor Doe, of Berne, was described. It consists of a hollow ivory cylinder, containing a smaller solid ivory cylinder, the latter movable, and connected at its upper end with two upright needles, which, when pressed upon by the solid cylinder, move like the hands of a barometer along a flat metallic indicator divided into equal parts, so that when touched or pressed upon, they move until the pressure ceases, when they remain stationary, registering the pressure, which is reckoned in grammes and millimeters ; the patient being laid down and the eyelids separated, the operator, by means of a silk thread, suspends the instrument by his teeth, and allows it gently to stand with its own weight on the outer side of the anterior surface of the eyeball, steadying it, but not holding it, by placing a finger against the side. The solid cylinder is allowed to project two millimeters below the level of the hollow one before being placed on the eyeball. So soon as the end of the projecting cylinder touches the globe, pressure is exerted, and the needles begin to register.

Proceeding to consider the cause of glaucoma, Mr. Walker, after mentioning the different views that had been held at various times, remarked that most probably the correct one was that held by Von Graefe, viz., that in all persons predisposed to glaucoma a rigidity and non-distensibility of the sclerotic exists, that this natural condition is increased and confirmed at

the approach of old age so that the ciliary nerves become pressed upon, their functions are interfered with, and the nutritive absorbent action of the parts they supply becomes affected, so that when slight causes of irritation arise the fluid contents of the eyeball no longer can be changed. The result is, that on the occurrence of inflammatory action no yielding of the sclerotic can take place, the products of inflammation are not absorbed, and increased tension follows. The most promising of the plans for relief at first employed, viz., paracentesis corneæ and intra-ocular myotomy have been gradually abandoned after repeated trials, and, in spite of strenuous opposition, the practice of iridectomy, first proposed by Von Graefe, has received the sanction of the foremost oculists of all countries.

The remainder of the paper was occupied with a review of the theories as to the rationale of the relief afforded by iridectomy. These all, however, tend to support the view that this operation acts by diminishing intra-ocular pressure, thus affording the restitution of conditions favourable to nutrition and circulation. Finally, Mr. Walker urged the early performance of the operation, as soon as a glaucomatous condition is fairly recognised.—*Liverpool Medical and Surgical Reports, Oct,* 1871, *p.* 141.

---

## 78.—UNRECOGNISED ACUTE GLAUCOMA—A FERTILE SOURCE OF BLINDNESS.

By Ernest Hart, Esq., late Ophthalmic Surgeon to St Mary's Hospital.

Perhaps the most serious class of cases in which I find on my note books indications of serious mischief having arisen for want of definite practical hints as to diagnosis, are the cases of acute glaucoma. Early diagnosis of this rapidly destructive disease appears, indeed, except in the hands of practised ophthalmic surgeons or of those who have attained special acquaintance with modern ophthalmic practice, to be still the exception rather than the rule. Yet there is no disease of the eye in which prompt action is so rapidly called for—none in which delay leads to more irretrievable injury. Of sixty-seven cases of acute glaucoma of which, during the last decade, I have noted details, which had been under treatment before I saw them, fifty-two did not reach my hands until they had suffered serious injury from delay; and in thirty-nine the true nature of the disease had not been suspected, and the treatment had been either useless or injurious in its character. This had not been due to any want of general capacity and skill, but to the fact that the attention of the professional attendants had not been

directed to this subject, and that they were not in possession of the few practical points which in this disease, as in others, suffice to establish *prima facie* evidence of the nature of the case, such as would at once have suggested the necessity of a more careful investigation of the facts. One or two instances will illustrate the subject, and I will choose some of the most striking. Five years since, I was asked by my friend Mr. Barnett to see a lady who had been suffering from an affection supposed to be connected with constitutional weakness and debility, for which she had been under treatment for some time. She had "inflammation" of the left eye, with loss of vision. When I saw her the conjunctiva was injected with blood; the cornea had lost its polish; the iris was ovally dilated; and, on closing the sound eye, it was found that in the inflamed eye vision was abolished. The ophthalmoscopic examination showed marked excavation of the disc, but great clearness of the media. The eyeball was hard and unyielding as a marble. It was a typical case of acute glaucoma, with total loss of vision from extreme intraocular pressure. There had been abundant premonitory symptoms—considerable increase of "far-sightedness" (*hypermetropia*), haloes seen around the lights in the evening, and pains in the eyeball. It was a great shock to those around when it was found that the operation of iridectomy afforded the only chance of recovery of sight, and that this must be performed without any delay, if it were to do any considerable amount of good. That opinion was, however, confirmed by Mr. Bowman; and on the same day I practised a full iridectomy. The sight was only partially restored, the other eye remaining good. The patient was well satisfied with the result, but did not cultivate the vision of the operated eye. Last January I was called again to see the same patient. She had been selecting silks in a draper's shop three weeks before, when she felt a severe pain in the eye, and the sight failed her suddenly. She returned home, and was placed under treatment. Once more she had calomel and opium, warm fomentations, and local antiphlogistic treatment, with general support by port wine, for she was very weak. Meantime, she was kept in darkness and in hope. When I saw her in consultation, I found sight totally abolished, as it had been during the whole three weeks; the conjunctiva red; the eyeball of stony hardness; the pupil ovally dilated; the cornea roughened; the media so extremely clouded that ophthalmoscopic inspection of the fundus was entirely obstructed. The neuralgic pains of the face and side of the head had been throughout and were still intense. Here was once more an attack of severe acute glaucoma; and this time it had been allowed to run on, unchecked by iridectomy, so long that the case was practically hopeless. Iridectomy afforded

the only chance. I informed her medical attendant of the true state of the case, and we once more had to make the best of a very awkward position. He assured her of recovery; and attributed the affection to ordinary inflammation with effusion, which would clear away. At this stage of the disease, even iridectomy held out but little chance of restoration of vision, and it could only be suggested as offering the one chance which remained. The result in this eye was not so good as in the other, where so much time had not been lost, and where the nervous structures still retained a power of recovery. I doubt whether this patient will ever have any useful vision with it; but the vision of the left eye, aided by stenopæic glasses, suffices for most of the ordinary purposes of life. I have seen the patient this week; she still has useful vision in the eye first operated on.

This case is perhaps calculated to dwell in the memory, and to fix there the lessons which it teaches, from the remarkable fact that in both eyes the attack was alike; that its true nature was similarly misinterpreted in both; and that the patient escaped the one error with partial vision in the eye affected, and the other with barely a remnant.

Not to be tedious, I will mention only one other case, which is less painful, but not less instructive. I was asked lately to see an aged gentleman, whom his attendant described to me as having been suffering for a little time from what seemed to be a troublesome conjunctivitis. There was in this case little or no pain —only some uneasiness of a very slight character. The patient insisted, and continued to insist, that he had a habit of sleeping with his face resting on his hand, and that he had unduly pressed upon the eye. His eye "felt a little bruised," but not enough to induce him to complain of it voluntarily. It was sensitive to light but not enough to prevent him from writing comfortably when it was partially shaded, or even from pushing the shade off the eye when writing; and he was but little disturbed about it. He was a hale old gentleman, but considerably more than eighty years old, and therefore already suffering from some infirmities of the prostate. This insidious and painless conjunctivitis was then an almost painless but marked glaucoma, and belonging pathologically, if not in all its clinical characters, to the subacute class. The conjunctiva was little congested; but the pupil was dilated, the eyeball of great hardness and tension, the cornea rough, the media dull, the optic papule cupped. On closing the sound eye, he discovered, much to his discomfiture, that he could not count fingers. Had I seen this old gentleman at the outset of his disorder, I should have counselled iridectomy, which alone could effect a cure. At his great age, and in the existing condition, in which iridectomy could

not be relied upon to give more than partial results, I thought it best not to urge any operation, but to lay plainly before his friends the facts that the sight was seriously compromised, that iridectomy might be expected partially to restore it, and that no other known treatment offered any such prospect; but that against this must be weighed the objections to any sort of operation involving confinement to a room and the inhalation of an anæsthetic on an aged man of a peculiar temperament. Ultimately it was decided, with the concurrence of his relatives, who included members of our profession, and of Mr. White Cooper, who kindly came into consultation, not to operate. Bromide of potassium, and nervine tonics and sedatives of various kinds, were carefully administered, and a soothing local treatment; but the disease was, as might be expected, unchecked, and the sight is lost in that eye.

It is not necessary, of course, here to speak of the usefulness of iridectomy in glaucoma. That belongs to the acquired facts of science and practice. Successful iridectomies in glaucoma are among the most striking, brilliant, and beneficent achievements of surgery; and iridectomy in glaucoma ranks among the greatest surgical discoveries of the age. Here I wish only to point out, from the results of my own experience, which has not varied in this respect during the decade, that the sphere of usefulness of iridectomy is narrowed by the delays which commonly occur in general practice in recognising acute glaucoma, and the omision to apply at once the one and only remedy—iridectomy.

The ophthalmoscopic test affords the crucial evidence of the disease, but it need not remain unsuspected for want of a few simple rules. 1. *Wherever there are superficial inflammation of the eye, spontaneous dilatation of the pupil (generally ovoid) hardness of the eyeball, pain, and diminution of vision, acute glaucoma may shrewdly be suspected.* 2. Wherever there is *spontaneous dilatation of the pupil*, accompanying slight redness of the eye, with *hardness of the ball and affection of the vision, subacute glaucoma may be suspected.* The previous history generally includes the following subjective symptoms: rapidly increasing far-sightedness, haloes seen around lights at night, and occasional pains in the eyeball. I leave out of view any ophthalmoscopic or other of the optical tests of circumscription of the field of vision, for experts will already be on their guard. The external characters to which I refer in these short rules have been more or less typically conjoined in all the cases which have come under my notice. I am satisfied that it will be very useful that they should, in the minds of all practitioners, be so associated with glaucoma (to which they belong) as always to suggest by their occurrence the probable nature of the disease, and to lead

to decisive investigation, and, if necessary, timely action. More eyes have been, and apparently still are, lost by the neglect or misapprehension of glaucomatous affections, than from any other single cause.—*British Medical Journal, Feb.* 3, 1872, *p.* 122.

---

## 79.—ON GLAUCOMA.

By Dr. H. R. SWANZY, Ophthalmic Surgeon to the Adelaide Hospital, Dublin, and late Assistant to Professor Graefe.

[The great importance of being able to recognise glaucoma depends upon the fact that if left to itself it is certain to lead to absolute blindness, but if proper treatment be applied in good time, nine out of ten cases can be saved.]

In what does this disease consist? It consists in an increased tension within the chamber of the vitreous humour, possibly a hypersecretion of the vitreous itself. What it is which gives rise to this hypersecretion, if such it be, has not yet been discovered, although numbers of experiments have been made on the subject, and theories without end propounded. The simple known fact however, is, that glaucoma consists in an increased tension within the chamber of the vitreous humour, or, as we more commonly say, an increased intra-ocular tension.

You may recognise the increased tension in the present case, as in all others of the disease, by palpation of the globe of the eye with the tips of your two index fingers, when you will find the affected eye much *harder* than the normal globe. In making this examination, I think it is well to support the 2nd, 3rd, and 4th fingers of each hand on the patient's brow over the eye, desiring him to look down at the same time. The pressure you apply to the globe must of course be very gentle, and it will be well for you at first to practice palpation of the globe on yourselves and your friends, so as to form an estimate both of the amount of pressure which you should exert, and of the average tension of normal eyes.

There are two chief forms of glaucoma, namely, simple and inflammatory. Our patient labours under the latter form of the disease.

Simple glaucoma destroys vision very slowly indeed, such a case often lasting two or three years. It is unattended by pain, and the only signs of the disease on the exterior of the eye are usually a few rather engorged episclerotic veins, a shallow anterior chamber (in consequence of the pressure in it being less than in the vitreous humour), and a sluggish pupil (owing to paresis of the ciliary nerves from direct pressure on them).

Patients then, labouring under this form of the disease, will come to you complaining merely of failing vision, you investigate the intra-ocular tension and find it more or less above the normal tension. You then examine the fundus oculi with the ophthalmoscope (a proceeding unattended with difficulty in this form of the disease, as the ocular media remain clear). The surface of the disc, or optic entrance, instead of corresponding almost exactly with the surface of the retina, as it does in the normal eye, is pushed back, so that the appearance of the excavated or cupped disc is presented. The surface of the disc is often driven further back than the external surface of the sclerotic itself. This cupping of the disc results from the fact, that it is the least resisting point of the coats of the eyeball, inasmuch as here the sclerotic is perforated by the filaments of the optic nerve as they enter the eye. In accordance with hydrostatic laws, the pressure within the eyeball must be equal on every part of its walls, and of course the weakest point of these will be the first to give way to an abnormally high pressure. In commencing glaucoma, we may find no cupping of the disc, as it requires the tension to have reached a certain height, and to have lasted a certain time before this effect of it is produced. The high pressure upon the disc and retina paralyses these tissues, and so produces disturbance of vision. The defects of sight display themselves in central vision, so that the patient cannot read so well, or distinguish small objects with his former precision; but, what is of much greater importance for the diagnosis, and in the long run for the patient too, are defects in the eccentric portions of the field of vision, which are apt to be present. They should be looked for, *by lamplight*, in the following way: —directing the patient to close one eye, you cause him to steadily fix the other one (to be examined) on some object at about two feet removed from it (e.g., your hand, or face); you then move a whitish object (your hand) slowly round in the periphery of his field of vision, and about one foot removed from its centre, and observe whether there be any position where the motion of the object can not be seen. If the glaucoma be somewhat advanced, you will almost constantly find a defect in the inner and upper region. At a still later period this defect may extend all round the periphery of the field of vision, and encroach also towards its centre, so that when the patient finds himself in a condition, which any of you may realise to yourselves, by closing one eye and looking with the other through a roll of music or other small bored cylinder. At last this little remaining glimpse of the world is swallowed up by the ever advancing enemy, and the patient is left in irremediable darkness. Or, the defect in the field may merely

advance towards the central point (point of fixation, macula lutia), and having attacked it go no further; such a patient would only be able to see objects held to one side of his field of vision.

Inflammatory glaucoma, of which you have here an example, has a very different course. It rarely extends over a term of more than a year, and usually but a few months. The tension is not a steadily increasing one, but advances in fits and starts, called "attacks of glaucoma." Each attack is attended by dimness of vision (often described by the patient as smoke in the room), by the appearance of rainbows round the candle or lamp, by severe ciliary neuralgia, which extends down the side of the nose, into the malar bone, and into the corresponding brow and side of the head, there is also much tearing and sensitiveness to light, and frequently vomiting. If then, you examine the eye during an attack of this nature you will find it very hard, the anterior chamber shallow, a bright injection surrounding the cornea, and much engorgement of the episclerotic veins. In consequence of pressure upon the ciliary nerves, the sensation of the cornea becomes greatly diminished, as you may discover by touching it with a small shred of paper. You may observe that in the case before us to-day, there is complete anæsthesia of the cornea. In these attacks of glaucoma, you will often find the aqueous and vitreous humours so opaque, that, even if the photophobia allow of the examination, the fundus oculi can scarcely be seen with the ophthalmoscope, the optic disc appearing but indistinctly, somewhat like the sun when seen through a thick fog. Such attacks vary of course in their intensity, from something which the patient himself hardly observes, to one like that I have described. An attack of glaucoma frequently lasts several hours. When it has subsided the media become clear again, the vision becomes almost as distinct as before, and the tension returns nearly to its former condition. Every attack, however, adds something to the permanent intra-ocular tension, takes something from the acuteness of vision, makes another advance towards cupping of the disc, and towards atrophy of the nervous tissues. As the disease gains a footing, the attacks become more frequent. Instead of returning once a fortnight they come once a week, then perhaps once every second day, or every day. At last an attack comes which has no complete intermission, although it may diminish in severity for a few hours, and when this happens we say that the glaucoma itself has arrived. The last moment for the exercise of your art will then also have arrived, for a few days would otherwise suffice to extinguish vision. If the disease be still let run its own course, although sight be completely lost, yet the most violent

ciliary neuralgia may continue for months. The eye is gradually disorganised. Cataract forms, and the iris becomes atrophied. Such is the condition of our patient's eye. Still later, the glaucomatous process leads to ecstacy of the sclerotic, purulent inflammation may then come on, and end in phthisis of the eyeball.

Glaucoma, of both forms, is most commonly double sided. Both eyes may be attacked about the same time, but more usually there is an interval of weeks or months, or even longer. In this man the second eye still continues healthy.

Until within the last fifteen years a successful treatment for this terrible disease was unknown. Patients suffering from it, and applying to a surgeon for aid, were simply allowed to go home and get blind. It is to Von Graefe that humanity is indebted for the great discovery that abcision of a portion of the iris (iridectomy) would relieve increased intra-ocular tension. In 1857 he published his first great monograph on the subject; and thousands are the eyes which have been saved since then by the operation. Graefe arrived at this discovery by a remarkable process of clinical observation, of which it would be difficult to convey to you an adequate idea. We are still ignorant as to how iridectomy has the effect of reducing increased intra-ocular tension. Doubtless, one day, this too will be discovered, and I think most likely then, when the cause of the increased pressure itself becomes known. In the meantime, we may be well contented to practise the treatment without being acquainted with its mode of action. How many medicines are administered internally quite as empirically?

In order to perform the iridectomy with a successful result, it is important you should attend to two points. 1. The portion of iris must be excised to its very periphery. 2. It must be a sufficiently wide piece. The first point is attained by making your incision into the anterior chamber, with the little iridectomy knife as far back as possible, i.e., in the corneo-sclerotic border. The width of the portion of iris excised will depend upon the length of the wound, because on withdrawing the knife, the aqueous humour flows out and carries with it a section of iris corresponding to the extent of the wound. This, then, is seized with fine forceps, gently drawn forward a little, and snipped off with the scissors close to its base.

The earlier in the disease iridectomy is performed the more perfect the result obtained, for, in general, we may expect by the operation to retain the degree of vision which exists at the time, but we can rarely calculate upon restoring what is lost. Eccentric defects of the field of vision do not recover after the operation; at best central vision may improve somewhat. You will find the results of the iridectomy in cases of inflammatory

glaucoma almost uniformly satisfactory. In cases of simple glaucoma, on the other hand, the operation sometimes appears to arrest the disease only temporarily, and you may afterwards have to employ a second iridectomy in order to complete the cure, and there are sometimes cases in which even that does not produce the desired effect, and where in spite of all your exertions the eyesight will be lost.

Glaucoma is a disease of old age, seldom making its appearance in persons under forty-five.

Before I conclude, I must mention another form of glaucoma, which fortunately is very rare, but for which you should be prepared, namely, glaucoma fulminans. It resembles inflammatory glaucoma, except that it has no premonitory stage, it gives no notice of its approach, but suddenly attacks an eye, which has apparently been until then, perfectly healthy. It comes on with great violence, and frequently destroys vision in a few hours. It consequently admits of no delay in the performance of the iridectomy.

I shall now proceed to operate on the case before you, not with any expectation of restoring a spark of vision to the eye, for the time for this is long since past; but in the hope of relieving the frightful neuralgia from which the patient is, and has been suffering for some months. Even in this I may not succeed; for, when the iris has become so atrophied as you see it here, we cannot speak of the result of the iridectomy with as great certainty as we otherwise could do. In case the pain be not allayed by the operation, I shall be obliged to resort to enucleation of the eye-ball.—*Medical Press and Circular, Nov.* 22, 1871, *p.* 453.

---

## 80.—CASE OF AMBLYOPIA.

By Dr. Donald Fraser, Paisley.

[The following is a case of white atrophy of the optic nerve, treated with the continuous galvanic current. It possesses a wider interest than pertains merely to the treatment of an obscure case of eye disease, for, as iritis and its treatment become the medium through which important lessons in the therapeutics of mercury are conveyed, so by means of the ophthalmoscope the effects of galvanism on certain affections of the optic nerve may ultimately throw much light on the treatment of nerve affections generally.]

I will now proceed to narrate the case to which I consider these remarks a necessary introduction.

William O., aged 59 years, consulted me on the 8th Sept., 1871, for failure of sight. He was, up till three years ago, a

weaver. He had been always a healthy man, and temperate. For the last ten years he smoked tobacco to the extent of two-and-a-half ounces per week. At the age of 44 he began to use spectacles for presbyopia. For the last 9 years of his work as a weaver, he taxed his eyes severely at pattern weaving, working most of the day, during the greater part of the year, in gaslight. For the last 5 years his sight had been gradually failing. It was not, however, until the beginning of last year that he began to be alarmed at the rapid increase of this failure. He now become dyspeptic, low spirited, and weak; all this due, he considered, to the depressing effects of some family troubles. At this time, during the day, a mist came before his eyes, which passed away at twilight, so that by gaslight he could read, for a few minutes at a time, the largest type of the newspapers with No. 6 convex glasses. About nine months ago even this became impossible.

When he called upon me he complained of a mist being constantly before his eyes, so that he was unable to recognise his most intimate friends above a yard off. I found that he could read slowly, and with effort, No. 20 of Snellen's test types at 4 inches from his eyes with the right eye, at 8 inches with the left, and at 6 inches with both. On examination with the ophthalmoscope, the outer two-thirds of the optic disc in both eyes was found to be pearly white in tint and glistening, and the inner third hyperæmic, the retinal veins were enlarged and tortuous, and the arteries diminished in number and calibre. Alongside some of the vessels were to be seen the white lines said to be characteristic of *neuritis descendens*. There was here then a markedly atrophic condition of the optic nerve, most probably primary; there being no certain evidence either by the ophthalmoscope, or otherwise, of intra-ocular causes sufficient to produce such atrophy. I dismissed the idea of the disease being due to tobacco poisoning, although, in some respects the condition of the disc seemed closely allied to what is usually described as due to excess in smoking. At the same time there was not, and never had been, any symptom of cerebral affection. Still I have been strongly impressed with the idea that this was a case of primary degeneration of the optic nerve, a degeneration which may find illustration in cases of so-called tobacco amaurosis.

Knowing how useless drugs were in such cases, it occurred to me that galvanism might be of use. But taking a serious view of the case, before using such an agent I advised my patient to consult Dr. Thomas Reid, of Glasgow. Dr. Reid confirmed my diagnosis, and while considering the case a very unpromising one, recommended the use of the bichloride of mercury in combination with iodide of potassium. My patient began this

treatment on the 10th September last, and continued it for a month. During this month he was a good deal at the coast, and came back to town much improved in general health. His sight, however, was scarcely, if at all, improved; although he could read No. 20 at 7½ inches. At this visit, I passed a current from six cells of a Stöhrer's battery through the temples for about twenty seconds. On testing his sight immediately afterwards, I found that he could read No. 20 at 9½ inches; an improvement of two inches within a period of as many minutes. I advised him, however, to go on with the bichloride mixture for another week or so. I saw him again on the 8th October, when I found, as I expected, his temporary gain had left him, he being only able to see No. 20 at 7½ inches as before. I may here mention that in reading, my patient always sought to make the best of his case, so that the limit of clear definition would be more correctly stated at an inch less than the above figure. I again tested him with the current, the result being an improvement of three inches. Two days afterwards he could read at 8½ inches. I again applied the current, and immediately afterwards he read at 10 inches. Feeling satisfied now that the galvanic treatment would yield good results, I asked him to cease taking the mixture, and to call upon me every morning at 10 a.m. In order to insure accuracy in the results, I kept him to the same hour, position as to light, &c. The days at this season and at the above hour, were usually dark and foggy; conditions, by the way, in which he could see best. On clear sunny days, he described the mist as being particularly thick and dark before his eyes. About a week after I began the galvanic treatment he improved in this respect, being able to see best on a clear day.

I continued the galvanization daily for a month, then every second day, sometimes every third day, and again every day as I thought fit. I sought to avoid the dangers of over-stimulation by the weakness of the current—six cells—and the shortness of the application, which never exceeded thirty seconds. I applied the electrodes at first to the temples, and to the long axis of the head, at each break of the current producing the flash indicative of retinal irritation. Latterly I applied one to the forehead, the other to the tongue, with alternations. Occasionally, and experimentally, I applied a current from eight cells to the cervical sympathetic,—the results, however, never seemed so good as by the other methods.

The treatment was continued for three months; during which time his progress was a matter of daily observation. At the end of that period he could read No. 5½ Snellen with as much ease as at the beginning he could read No. 20. The improvement in his health and spirits due to this progressive recovery of

sight has been very marked. It may be here observed that every day there was a distinct improvement of one inch in his ability to read the test-types. This improvement, which affected both eyes, was usually to the extent of two and three inches after the application of the current, though the permanent effect was only one inch.

I have demonstrated a certain amount of improvement by means of the test-types. The evidence of the ophthalmoscope may now be added. On the 19th November, 1871, I fancied, on examining the fundus, that the condition of the disc was, if anything, slightly improved. On the 11th January, 1872, I made a very careful examination, and was satisfied that while the atrophic condition of the disc was still very marked, there was an improvement. This was particularly so in the case of the retinal circulation, the arteries being distinctly larger in calibre, while the veins were smaller and much less tortuous. As it was an important point that there should be no doubt as to this improvement, I again asked my patient to see Dr. Reid, who substantially agreed with me.

The question now is, how far this improvement will go. I do not expect that much more can be effected in the case, although I mean to continue the treatment twice a week or so for some time to come. A more important question is, how far will the results be permanent? My impression is, that with care on the part of the patient his present condition may be maintained for a considerable length of time, if not permanently.

But whether this be so or not, it is evident from the ophthalmoscopic examinations, that we have not simply stimulated the patient's retina, but that we have distinctly improved its circulation, as well as the nutrition of the optic nerve, and that this has been done in a disease in which ordinary medical treatment is practically useless.

It seems likely that in this case the galvanism acted as a direct stimulant to the optic nerve, and that the improvement in the retinal circulation followed secondarily, in great part as the result of the improved nutrition of nerve, and partly from the direct action of the current upon the vessels of the retina. The idea that the influence has been mainly exerted upon the nerve is impressed upon me by the results of similar treatment in a case of so-called atrophy of the optic disc, depending distinctly upon intra-ocular changes, probably choroiditis. The patient was a delicate lad, 18 years of age, who had suffered from some severe inflammatory affection of the eyes in childhood. In this case not the slightest improvement was observed (or indeed scarcely expected) from a very careful galvanization, conducted as in William O.'s case, and continued for many weeks. Now if the retinal circulation had been affected in this

case, as it is found to be in galvanization of the cervical sympathetic, some improvement might reasonably have been expected. It is possible that in cases of this kind, with a continuous course of galvanization of the sympathetic, in conjunction with other treatment, good results may yet be obtained. But for those purely nervous degenerations of which W. O.'s case is a type, I am firmly persuaded that the continuous galvanic current will be the appropriate remedy of the future.—*Glasgow Medical Journal, Feb.* 1872, *p.* 163.

---

## 82.—ON THE FORM OF AMAUROSIS SUPPOSED TO BE DUE TO TOBACCO.

By Jonathan Hutchinson, Esq.

[Mr. Hutchinson has twice before published his experience on this important subject. One of his former papers will be found in *Retrospect*, vol. 1, p. 274.]

Any one who may take the trouble to compare the cases given in these reports, will be struck by their similarity, and by the remarkable confirmation which the later papers give to the conclusions of the first. A very great disproportion in the numbers of the two sexes who become the subjects of "idiopathic amaurosis," was one of the facts brought out (for the first time) by my first report. In it the numbers were 37 men and 3 women, and in the second they were 34 men and 5 women. In the present one they will be found to be 28 men and only one woman. There can, therefore, be no reasonable doubt that we have to deal with a formidable type of symmetrical amaurosis which is almost exclusively confined to the male sex. For myself, I may briefly avow that I have scrupulously investigated other possible causes, and that I feel no hesitation in believing that in most of these cases tobacco is the real one.

The facts as regards the one woman whose case is included in the present series, are extremely interesting. Her case in every feature resembled those which in males I attribute to smoking, and upon this point I had made special comment to those who were attending my class. After I had done so, she informed me that one of her sons had formerly been my patient, and that a nephew had been under the care of Mr. Hulke. I referred to my notes and Mr. Hulke was kind enough also to refer to his, and we each found that the cases had been diagnosed as tobacco amaurosis. In each the patient was a young man and a smoker. Now the lesson of these facts seems to me to support the opinion I have long held, that when tobacco causes blindness it does so in virtue of an idiosyncracy. It is by no means improbable that such idiosyncracy will be found occasionally

in several members of the same family, and further that it may involve liability to suffer from other influences besides tobacco smoking. Thus, then, we have two young men, cousins, attacked by amaurosis at an early age in virtue of an idiosyncracy rendering them liable to special poisoning from tobacco ; and we have a woman, who had never smoked, the mother of one and aunt of the other, at a much later period of life becoming the subject of a similar form of nerve disorder in virtue of the same peculiarity of diathesis acted upon by a different exciting cause. In her, not improbably, the exciting cause was the disturbance consequent upon the cessation of menstruation.

Holding the opinion that there must be some pre-existing peculiarity in the nervous systems of those who become the subjects of tobacco amaurosis, I have been very anxious to discover if possible whether it reveals itself by any other signs. The only points in this direction to which I feel at present inclined to attach any importance are the following. Not unfrequently it will be found that these patients have had unusual difficulty in learning to smoke, and have throughout life displayed special susceptibility to its influence, and that also they have often been beyond the average liable to suffer from sea sickness. In the tabular statement some information will in many cases be found given on these points. My attention to the matter of sea sickness was first given in consequence of the statements of some patients who were sailors, and who specially referred to it.

We have, during the last few years, made the observation, that those who suffer are, almost invariably, smokers of *shag*, the most deleterious form of tobacco. As regards the results from disuse of tobacco my impression is strong, that almost invariably when the disuse is real and complete, the state of vision improves. I cannot exaggerate the expression of my conviction as to the duty of urging immediate and complete abstinence in the early stages of this most serious malady. Many of my patients came too late, having continued to smoke until the disease was far advanced. I have never seen a case in the early stage in which the disease went on to blindness if the patient had strength of will to give up the habit. In a large majority of those in whom, according to the patient's account, the habit was wholly abandoned, I had reason to suspect that it was only reduced. My experience on this point fully confirms that of Dr. Mackenzie, that there are those "who would rather smoke than see." It is very few, however, who have the honesty to admit their inability to give up the habit, and hence a very annoying source of fallacy in our inferences as to the effect of treatment.—*Ophthalmic Hospital Reports, Nov.* 1871, *p.* 169.

## 83.—AMAUROSIS SUCCESSFULLY TREATED WITH HYPODERMIC INJECTIONS OF STRYCHNIA.

By A. S. G. Jayakar, Esq.

P. H., a Rajpoot, aged about 35, was admitted into Hutteesingh's Hospital, on March 21, 1871, with total amaurosis of both eyes. He had an amaurotic look, and his vision was very dim, being perfectly unable to distinguish light from darkness. He constantly turned the eye-balls upwards, almost under the upper lids, evidently for the purpose of attempting to bring the remaining small portion of the retina in axis with the object seen.

*History.*—The history of the case was rather unsatisfactory, the patient being far from intelligent. The only account he could give of his blindness was that it first commenced thirteen months ago with a sensation of burning in both the eyes, followed by a gradual loss of sight. He then suffered from fever for about ten months, and had suffered from total amaurosis for nine months before his admission.

*Ophthalmoscopic Examination.*—Right eye: image natural; media dull; disc anæmic; no venous congestion; shape of blood-vessels undefined; arteries atrophied: retina pale and anæmic. Left eye, image natural; media dull; disc and retina anæmic, the same as in the other eye; arteries atrophied.

There was no photophobia or pain of any kind in either eye.

On March 31 one twenty-fourth of a grain of strychnia was injected into the back of the neck.

April 1. The patient expressed himself slightly relieved. He could count fingers and distinguish colours with the left eye. With the right he could only distinguish light from darkness.

4th. There was some more improvement in the right eye. He could count fingers and tell different colours with it; but there was no progress with the left eye. He was ordered to again have one twenty-fourth of a grain of strychnia injected.

5th. Left eye considerably improved; so also the right, to a certain extent. Could tell the shapes of different objects and count dots.

9th. One twenty-fourth of a grain of strychnia was again injected.

10th. There was a marked and decided improvement in the left eye, the right also having improved to some extent. With the left he could tell minute objects.

18th. Another injection administered.

19th. The patient could count dots No. II. (arranged on Snellen's test-type scale). With the right he could only count No. XX.

May 21st. An ophthalmoscopic examination revealed an improved state of both the disc and retina in the left eye; whilst the same structures in the right eye were in much about the same condition as on his admission.

7th. Hypodermic injection of strychnia was given in the right temple.

12th. The injection in the right temple was repeated.

22nd. No change having being noticed in the right eye since the last note, the injection was again repeated, with this beneficial effect: that he could see No. VI. easily with the right eye, the sight in the left eye remaining stationary.

By June 6 (the day on which he was discharged from the Hospital, being very anxious to return home) the physical condition of both the disc and retina in the right eye had greatly improved; and, instead of atrophic arteries, there were well-defined bloodvessels in both the eyes. Vision of right eye could count No. VI.; with left eye could count No. II.

*Remarks.*—This was evidently a case of asthenia of the retina and discs, accompanied with exhaustion of the optic nervous apparatus, and a case, therefore, best suited for treatment with hypodermic injections of strychnia, so strongly advocated in these cases by Professor Nagel, of Tübingen. The patient had altogether seven injections between March 31 and May 22, the quantity of strychnia used being about one-third of a grain; and, on the whole, the progress of the case had been satisfactory. Perhaps, had a larger quantity been injected each time, a more rapid improvement might have been noticed; and, if the line of treatment had been persevered with for a few weeks more, I have no doubt but that his right eye would have eventually come to the same state as the left, so as to be able to count No. II., and even some further improvement may have been reasonably expected in the left.—*Med. Times and Gazette, April* 6, 1872, *p* 398.

---

## 84.—SUBSTITUTE FOR THE OPHTHALMOSCOPIC MIRROR.

By Dr. J. S. Torrop, Heywood.

The ophthalmoscope having come into general use in medicine, it may save considerable embarrassment in choosing from among the vast variety of instruments offered, to point out that we all possess one already in our watch-pockets—viz., a watch-glass.

Any transparent reflecting body is an ophthalmoscope, and, with the ordinary lamp, quite enough light is returned from the concavity of a watch-glass to display all the details of the fund of the eye. It is used in the same manner as the mirror, but possesses this advantage that, (in viewing the inverted image)

it may be held at any convenient distance from the observer's eye, so long as he looks through it. Therefore, if more light be desired, the watch-glass may be approximated to the patient's eye till all its reflection is concentrated on his pupil. Atropine is not required except in special cases, as there is no glare to provoke contraction of the pupil.

Any watch-glass will do provided the glass be (as it is in almost all of them) equally thick throughout, so as not to distort the image seen through it. Improved forms will, of course, at once occur to the reader—e.g., with a central convexity to enlarge the image or concavity to correct myopia; but to discuss these would be foreign to my purpose, which is merely to point out the use of what we nave already got; the employment of which, I may add, was suggested to me by the reports of some of Mr. Carter's admirable experiments.—*Lancet, March* 2, 1872, *p.* 309.

---

## 85.—ON POLYPUS OF THE EAR.

By Dr. W. B. Dalby, Lecturer on Aural Surgery at St. George's Hospital.

[It will generally be found that polypi of the ear have been preceded by disease of the middle ear, which has resulted in a perforation of the tympanic membrane, purulent catarrh with scarlet fever being the commonest of all forms. This rule is, however, not invariable, for they are sometimes found after death as a commencing growth in the cavity of the tympanum, and sometimes they appear in the external auditory meatus independently of any tympanic disease.]

They are at times very vascular, and when they are so there is a good deal of bleeding after their removal. When they do not attain any considerable size, a very common appearance for them to present is a red, fleshy, globular mass projecting through a perforation in the membrane. In such a case they grow from the cavity of the tympanum, and from their situation, by preventing the escape of discharge, sometimes thus become the indirect cause of death in those fatal cases where the lateral sinus and brain become involved.

The reasons for which their removal generally is demanded are the continuous offensive discharge from the ear, and deafness, both of which symptoms are their invariable accompaniments. The most usual position from which aural polypi arise is some part of the walls or roof of the tympanum; they also spring from the external meatus and from the tympanic membrane itself. After one polypus has been removed from the cavity of the tympanum, it not unfrequently happens that one

or even two more are brought into view. In looking down a speculum where the membrane has been completely destroyed it is not always easy to decide at first sight whether the red mass at the bottom of the meatus is a small polypus, or simply granulations on the lining membrane of tympanum. This however, is ascertained by examination with a probe, when a polypus will be found to move under the touch. Indeed, this should always be practised so as to determine, as nearly as possible, the point from which the growth arises.

The indications for treatment of aural polypi are threefold. Firstly, to remove them; secondly, to apply caustics to their root regularly and for a sufficiently long time, to ensure against their reproduction; and, thirdly, when they exist with a perforation, to keep the tympanum clear of secretion, and to induce a more healthy condition of the lining membrane of this cavity and the Eustachian tube; in short, in addition to directing attention to the polypus, the case must be treated as an ordinary perforation of the tympanic membrane.

It is most desirable that the tediousness of these cases should be recognised. Although, after removal, a few applications of some caustic will occasionally eradicate them, and this especially when they arise from the external meatus, the treatment will often extend over several weeks, or even months; the time occupied depending upon the tendency to reproduction which the growth manifests.

Judging from the cases which present themselves where polypi were taken away some years ago and have recurred, only to be again taken away and again to reappear, my conviction is that until late years the perseverance necessary for the successful termination of this class of cases was but imperfectly estimated.

With regard to removing the polypus. If it be very large, the best instrument is a Wilde's snare armed with fine gimp. If of moderate size or small, simple rectangular forceps, meeting at the points by two rings, are the most convenient. The lever ring forceps of Toynbee are not often necessary, and at the best will only grasp a very small growth. While using any kind, or in applying caustics, the meatus should be illuminated with diffused daylight through the largest speculum that it will hold, the reflector being worn on the forehead and fastened with a band round the head; both hands will thus be quite free. The most effective caustic I find to be chloro-acetic acid, applied on a very small camels'-hair brush. In using this, care should be taken not to touch the meatus; and provided this is done, very little pain is felt; but if this should happen, it is relieved at once by syringing with warm water. Potassa fusa and chloride of zinc are not so manageable, and the

former is liable to spread on to more parts than it is necessary should be touched. Nitrate of silver is not sufficiently powerful, but it is a very useful caustic to apply to granulations on the membrane or on the surface of the cavity of the tympanum. The liquor plumbi is useful for the same purpose, and may be applied with a camels'-hair brush, or in the form of a piece of cotton-wool soaked in it and worn for a few hours every day pressed on to the part. It is well, before any of these applications, to dry the part carefully with cotton-wool.

A plan for cleansing the middle ear of secretion and applying solutions to the whole extent of the lining membrane in cases of perforation, by forcing them through the tympanum and Eustachian tube by means of a syringe with the nozzle guarded by india-rubber, and fitting the external meatus, was described by me in the Lancet in April and August, 1870; and in all cases where there is a perforation of the membrane and a pervious Eustachian tube, it is most useful. The patient may also be instructed to practise a plan which I first saw Dr. Joseph Gruber at Vienna teach at his clinique to patients with perforations—viz., to incline the head to the affected side; and, after a drachm or so of the lotion to be used has been passed into the inferior nasal meatus of the same side with a small glass syringe, to close the nostrils firmly with the fingers, and the mouth also being closed, to blow vigorously, when the fluid will run through the Eustachian tube, and out of the perforation; thus cleansing the lining membrane in its course. When neither of these plans succeed—and occasionally they fail on account of some temporary obstruction, — the Eustachian cathether may be employed with an india-rubber bag fitting into its open end; and thus fluids may be made to pass directly through the Eustachian tube into the tympanum, and through the perforation. Speaking generally, however, I should like to be understood to imply that no pains should be spared to keep the cavity of the tympanum thoroughly cleansed; and, in case of an adult, no trouble which has this in view should be grudged by the patient. He should be provided with a syringe which he can use without fear of hurting himself, and with this he should wash the ear twice a day. Lotions (a very good one is sulphate of zinc and solution of opium, four grains and ten minims respectively to the ounce) should be used in the following way:—The meatus is filled with the lotion, previously warmed, and with the affected side uppermost and horizontal, let the patient blow through the perforation; this will cause the fluid to bubble in the meatus, and, on ceasing to blow, some little of it will pass into the throat, and thus it will come in contact with all parts of the lining membrane of the middle ear. Observation teaches me to be

more hopeful as to the results of treatment in cases of perforation when they are accompanied by polypi than when they are not. I cannot in any way account for this, any more than I am able to do—judging simply by appearances as regards size, shape, &c.—for the immense variations in improvements to hearing that are met with after treatment in all cases of perforation. When the lining membrane of the tympana and Eustachian tubes becomes more healthy, the improvements that take place, with regard to the hearing power, are with some very great indeed, while with others they are scarcely perceptible. With these latter a careful examination with the tuning-fork, the history, and some subjective symptoms — tinnitus, worse hearing after fatigue, &c. — will frequently detect a nervous lesion accompanying the tympanic disease.—*Lancet*, *Dec.* 9, 1872, *p.* 809.

---

## DISEASES OF THE SKIN.

---

### 86.—RINGWORM IN SCHOOLS.

By Dr. TILBURY FOX, Physician to the Department for Diseases of the Skin, University College Hospital.

[The management of ringworm in private practice, is by no means a simple matter, and in large schools it is of course far more difficult and involves a good deal of responsibility, as a mistake may involve the principal in serious annoyance and pecuniary loss. Dr. Fox's attention has lately been drawn especially to this subject by an outbreak in a large school near London, in which 300 cases of the disease occurred.]

*Origin and Dissemination.*—Firstly, I do not understand that a school is properly managed unless every child admitted is shown to be free from ringworm of the head ( tinea tonsurans) and ringworm of the body (tinea circinata), either as certified by a medical practitioner, or by a careful examination at the time of admission by some competent person. A matron or good nurse can have no difficulty in preventing the introduction of ringworm as the rule. Every child with ringworm (tinea tonsurans) has certain " scurfy patches" or spots where the hair looks shrivelled or unhealthy. Such appearances and red scurfy patches on the body are readily detected by any one who has a pair of eyes, and they should suffice to excite suspicion and to lead to medical examination. Secondly, every week at least a careful inspection of heads should be made in schools. The heads of girls should especially be searched on account of their long hair. In this way the earliest signs of disease must be detected. Ringworm of the body should be recognised more decidedly than it is as the frequent source of ringworm of the head,and dealt with accordingly.

Well, once introduced into schools, how is ringworm spread? (*a*) By neglect, of course; (*b*) by actual contact of the healthy with the diseased; (*c*) by the common use of towels and brushes by the diseased and healthy; (*d*) by the air of the institution, which, under certain circumstances, is loaded with the germs of the fungus—trichophyton tonsurans.

I want specially to notice the last point. When I came to collect the dust deposited from the air in the wards of the institution in which the outbreak of ringworm before referred to occurred, I found that it contained fungus elements in abundance. This observation I believe to be a novel one. The achorion has been detected in the air passing over children affected with favus, I know; but I speak of the existence of the trichophyton in the air when no artificial means have been adopted to disseminate it there.

[This abundance of the fungus of ringworm no doubt arose from the large number of children collected together—of whom their were 121 in one room. The dust contained abundant spores of trichophyton, which were seen to develope into mycelium two days subsequently.]

*Treatment.—As regards the actually diseased.*—Isolation at all hazards is the first thing to do. I will only say on this point that cases of ringworm of the body must be isolated. I think this of essential importance in the case of schools. It is not, however, thought of any moment as the rule. Where a number of cases occur, it is better to separate instances of very bad and extensive diseases again from slight *new* cases and convalescents, for the simple reason that active treatment may at once annihilate the disease in the former, and in the new cases and convalescents fresh implantations over the, in the main, healthy area of the scalp may be taking place from contact with bad cases of tinea. I would, of course, only adopt this plan where the cases of disease are very numerous—say thirty, forty, and fifty or more.

There are, next, certainly general considerations to be taken account of. Attention to the dietary is one; for the underfed, and ill-nourished, and ill-kept furnish the most appropriate nidus for ringworm. All deficiency in meat should be rectified, and in case the attacked or the non-infected look sickly or pallid, the allowance of meat and fresh vegetables should be increased and supplemented by iron and cod-liver oil. So, again, the cubic space allotted to each child should be ample, ventilation free, and cleanliness enforced with exceptional strictness. One word more as regards the general health of children. If with a rigorous system of inspection in constant operation many cases rapidly appear, and, in spite of hygienic measures, spread, the children furnish clearly a very suitable soil, and the dietary of the children should be looked to. If

ringworm becomes epidemic, with a *bad system of inspection*, it implies simply neglect, of course. Here isolation is the main thing needed to protect the healthy, and not feeding up.

The treatment must be very briefly referred to. In all cases in schools the hair should be cut short, close to the scalp. Recent cases are at once checked and often cured by simple blistering. The disease, not having reached the bottom of the hair-follicles, is at once accessible to remedies. Cases of ringworm may be dealt with in the same way. The use of strong acetic acid is perhaps as good as the blistering fluid. If the case is not very recent, epilation of diseased hairs, after the Paris fashion, should be practised. It is generally "too much trouble to do this." I next enforce the use, every few days, of Coster's paste to the extent of some five or six applications, and the subsequent use, night and morning, of some parasiticide ointment, diluted citrine ointment, or sulphur, creasote, and ammonio-chloride of mercury. The head should be washed each day and well greased. The latter prevents the escape and dissemination of fungus germs. If preferred the head may be kept soaked in diluted sulphurous acid; of course a proper cap of silk should be worn.

*As regards the surroundings and belongings of the attacked.*—It is scarcely necessary to do more than refer to the necessity of thoroughly cleansing the brushes, combs, and towels of the diseased, and seeing that these are not used in common by the healthy and the infected. Towels should be well boiled. To one novel point I must direct special attention. It is the disinfection of the air of the wards in which a large number of cases of ringworm have been. My recent observations show that the fungus germs are floating in the air, and though I had until lately no experience to go upon, because the observation is a novel one, yet I have no hesitation in saying that the air of the wards should be disinfected by burning sulphur if, after complete isolation has been practised where many cases of ringworm have occurred, other instances of disease still continue to appear amongst the previously healthy.

*The prevention of new outbreaks.*—*As regards the reimportation by those apparently convalescent.*—No more puzzling problem is presented to the practitioner than that of saying when a child "is well of ringworm," and "fit to go back to school." I err on the side of caution if there is the least doubt, and advise that the same course be taken by others. When a child is well—that is to say, incapable of reimporting or redisseminating the disease amongst his fellows—there will be present certain naked-eye characters and microscopic appearances. The hair will be growing vigorously and naturally in the original sites of the disease; there will be no scurfiness, no broken-off hairs,

and the structure of the hair and its sheaths will be properly developed and free from fungus elements. If the hair is dull and dry, suspicion should be excited; and if the suspected surface is *studded over with short broken-off hairs* (*readily overlooked*), there is still disease present. The fungus will be formed in abundance in the short broken-off hairs. As a rough guide, this is the best. Condemn a child any portion of whose scalp is studded with the little dark points of short broken-off hairs. This is the rule I observed. But no one can be certain in any case without a microscopical examination. If the root is well formed, and the hair sheaths likewise; if fenestrated membrane can be seen, and no fungus detected, then all is right. Of course, fungus in any abundance is at once discovered. The doubtful cases are those in which the root seems healthy, but the shaft of the pulled-out hair is observed to be surrounded at its follicular portion above the root with epithelial and exudation matter. This may be an indication that irritation is being set up by the remedies, the ringworm itself being well. I see many cases of this kind, and in them the roots and surrounding structures, and hair-shaft, are healthily formed, whilst no fungus elements are to be detected in the material surrounding the hair. The scalp in these cases is tender, more or less swollen, and reddened, the hair at the same time growing well and vigorously. Perhaps the plainest and easiest guide to disease still existing, is the presence of short broken-off hairs.

*Rekindling of the disease from special causes.*—In order that ringworm may not "break out afresh" in schools, the non-infected must observe all those directions which were referred to under the head of "dissemination of the disease." Especially it is important to keep heads perfectly clean by frequent washings, and to keep them fairly greased or oiled. To this latter point I attach much importance.

This, in short, is a sketch of the means to be followed in managing ringworm in schools. There are those who think the use of a weak parasiticide to the healthy is advisable. Well, there can be no objection to sponging the heads, even daily, of the healthy with diluted sulphurous acid, one part to six of water, or better, with diluted acetic acid, one part to four or six of water.—*Lancet, Jan.* 6, 1872, *p.* 5.

---

## 87.—ON THE TREATMENT OF ECZEMA.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases, Dublin.

*External Applications.*—In acute or very slight cases of eczema almost any mild astringent will suffice. Perhaps among the

best we may rank subnitrate of bismuth in elderflower-water or camphor mixture, or liquor of the diacetate of lead, two drachms to six ounces of either fluid. When expense is not an object half an ounce of glycerine should be added, as from its faculty of retaining moisture it powerfully aids the purpose which lotions are intended to serve.

℞. Bismuthi subnitratis, ʒ ss.; glycerinæ, ℥ ss.; spiritûs lavandulæ, ʒ iij.; aquæ flor sambuci, ℥ vij., ℳ ft. lotio.

℞. Liquoris plumb. subacetatis, ʒ ij.; spiritûs rectificati, ʒ iij; glycerinæ, ʒ iv.; mist. camphoræ, ℥ v. M. ft. lotio.

When the essence of camphor can be procured it may be substituted for the rectified spirits.

When there is a large weeping surface, particularly in children, or of long standing in adults, the reader may try Dr. Hughes Bennett's plan. It consists in applying a solution of carbonate of soda, half a drachm to a drachm in eight or ten ounces of water, by means of a piece of lint soaked in the liquid and laid upon the part affected. The lint must be covered with thin gutta-percha or oiled silk to prevent its getting dry, or else the management of the affair must be entrusted to the care of some person who will see that the lint is kept moist, for which purpose it should be sprinkled continuously with cold water, the lotion being only occasionally applied. If this plan be properly carried out, if the linen be kept wet all day long, and still more if this can be effected day and night, the results are often marvellous; but if it get dry the soda speedily irritates the skin. The addition, however, of glycerine, half an ounce to eight ounces of fluid, will mitigate this to a certain extent, but nothing compensates for want of attention. Dr. Wallace has reported some cases in which the beneficial results of this plan were very marked. My experience of it is, that though it gives great relief, yet it has very little curative power, that it is only suitable for a large wet surface, and that it is apt, in eczema of the head, to give a bad cold.

In chronic eczema a lead lotion, the same as above, can be used. In many cases, where the surface is very irritable, it answers best when gently warmed previously. So soon as the discharge and inflammation are checked, zinc ointment forms an admirable dressing. It should be gently melted down, or rubbed down with an eighth part of spirit of camphor, and smeared like thin cream on the part. All surfaces to which ointment is applied should also be covered with old linen. When once ointments are begun with no more watery applications should be used, nor should the part be washed, except when the eczema is seated on the head and discharging freely, as happens sometimes, especially in children, or when in parts where washing cannot always be avoided, as the hands, face,

&c. At such times the discharge, when the eczema assails the head, is sometimes retained under the crusts, or the hair gets matted with it. Here it is very useful to poultice the crusts with mashed turnip or bread and water, till they are thoroughly softened, and then remove them by very gentle washing with hot water and yolk of egg, or what I prefer to anything else, the St. John's Hospital soap. Mashed turnips is the best poultice I know of; it possesses the great advantage of rapidly removing any unpleasant smell. In some cases a weak lotion of chloride of zinc, a grain to an ounce, with the addition of half a drachm of mucilage, is a very valuable application to the head; it is only here that I have found the chloride of zinc useful. But for the entire removal of eczema in the dry stage we must turn to a more potent remedy—one of the nitrates of mercury. I have never seen the oxide of zinc ointment—even that prepared by Messrs. Bell, of Oxford street, really cure this disease when severe; and I have repeatedly treated in the same patient, one patch of eczema with the dilute nitrate of mercury ointment, and one with the oxide of zinc, the result being invariably most decidedly in favour of the former; I have also made similar trials with the ointment of the nitric oxide as against that of the yellow nitrate, and am disposed to prefer the latter. Indeed, in my hands, the ointment of the yellow nitrate diluted, has proved superior to any application I have seen tried. Two or three years ago I showed several surgeons the effects of its action compared with the chloride of zinc as recommended by Dr. McCall Anderson, and the zinc ointment. Three patches were selected on the same patient, pretty nearly of like size and in the same stage. One was treated with the solution of the chloride, one with the zinc ointment, and one with dilute nitrine ointment. At the end of a few days it was manifest to every one that the action of the nitrate was superior to that of the zinc ointment, and this again to the action of the chloride; and by the time that the patch treated with the nitrate was healed, that to which the zinc ointment had been applied was much better, while that treated with the solution might be roughly computed, so far as such computations hold good, at only half way towards a cure. These applications were now abandoned, and the diluted nitrate was alone resorted to, under the influence of which the progress of the two remaining patches was soon visibly accelerated.

But if the application is to be useful it is indispensable that it should be properly made up and properly employed. In the first place, pure, well-made nitrate of mercury ointment only should be selected. A great deal of that which is generally used is totally worthless, being dry, dirty green, and rancid,

spoiling almost as soon as it is diluted; whereas, when properly prepared it retains its bright yellow colour for months. The dilute ointment should only be prepared when it is wanted, and the best plan is simply for the patient to rub it down with a little sweet almond oil till it is of the consistence of cream, and always to throw away what is left. If the eruption be seated on the head in children, the hair should be cut off and the ointment applied night and morning; a linen cap should also be worn day and night. But grown persons cannot, or will not submit to this; the hair should, therefore, be parted, any crusts gently detached, and the ointment rubbed carefully, but not roughly, in, the cap being worn at night only. Should the hands be affected soft leather gloves with the tips of the fingers cut off, should be worn during the night, and, if possible, during the day also. Where there is very little hair, and the eruption is only slight, the ammoniated mercury ointment of the "British Pharmacopœia" may be tried; it is one of the cleanliest and least offensive preparations that we possess, but in point of efficiency I would place it decidedly below the nitrate ointment. Some persons with a strong tendency to eczema, or after the cure of the worst part of their complaint, suffer a great deal from a chapped, red and tender state of the skin, cold cream is often very useful in relieving this. A powder composed of equal parts of finely ground American corn flour, and oxide of zinc, with camphor, dusted over the face during the day time, especially if the patient is to be exposed to a cold wind, is also of great service. Whatever theoretical objections may be made, ointments cannot be too sedulously employed in eczema even during the day, and when the eruption is seated on the face and hands. When the hair is falling from eczema, I know of no remedy so effectual as cutting it quite short, and blistering it as for alopecia. With regard to the itching I never yet saw any remedy materially affect it. The cure of the itching is the cure of the disease. The same symptom holds good of another disagreeable but rare symptom —a kind of neuralgia or rheumatism of the skin sometimes seen in eczematous patients. Mr. Wilson says he has found no remedy for it equal to a solution of nitrate of silver, a grain in an ounce of spirit of nitric ether.

Dr. McCall Anderson seems to think the lard is the chief agent in the benefit said to be effected by mercurial ointments. It happens that I not only have subjected, but am every day subjecting, this doctrine to a test; for it is a constant practice with me to prescribe, at the same time, pure lard and ointment of the nitrate of mercury, the former to be used when the nitrate sets up much irritation, and I have repeatedly had occasion to

satisfy myself that the ointment is much more powerful than the lard.—*Medical Press and Circular, Nov.* 29, 1871, *p*. 479.

---

## 88.—CARBOLIC ACID IN PARONYCHIA AND STINGS.

By Dr. Alex. E. McRae, Fettercairn.

I have very frequently treated wounds by carbolic acid ; not entirely according to Mr. Lister's elaborate system—for in country practice it is not always practicable—but by the more simple plan of syringing the wound, or washing the surface with a weak solution in water or in oil of the acid, and dressing it with the same. Its remarkable success over every agent I had ever used, or seen used, led me to use it in the following cases as a forlorn hope. The results obtained must be my apology for this record.

*Case* 1.—T. C., an officer of dragoons, had been suffering from whitlow of the last phalange of the right thumb for nearly three weeks. The thumb was very much swollen : but the first phalange was much more so, and redder than the last, although in the last the pain was very much greater. The wrist also was affected, and the glands of the axilla were tender and swollen. He appeared to have suffered a great deal, and was much weakened by want of sleep and of his customary recreations. He had tried several times to prick the swelling, but never went beyond the cutis vera. Much against his will, I laid the swelling freely open from the middle of the first to the tip of the last phalange, and a good deal of bloody pus escaped. For the next three weeks the wound healed greatly ; a very offensive ichorous discharge of blood and pus now came away, and the wound looked unwholesome. On examination, the probe distinctly grated upon the bone, without any pain ; but whenever the tissues alongside the bone were touched, the pain was very sharp. The relative merits of excision and amputation were considered. Before proceeding to either, however, I determined to try carbolic acid, by syringing several times a day; and the wound healed up in fourteen days. As the tissues were very loose, I applied a coating of collodion, and made a slit in the envelope thus produced, opposite the wound, as a vent for the discharge.

*Case* 2.—W. McA., a gamekeeper, healthy and robust, was bitten in the last phalange of the first finger of the right hand, by a tailor, in a drunken brawl. The marks of the teeth were on each side, and seemed deep; the parts were painful. In a few hours the pain increased, inflammation set in, and an unwholesome discharge commenced. The finger, hand, and arm,

and ultimately the glands in the axilla, became much swollen. In about a month, the probe could be passed through a sinus from the one opening to the other, grating on the face of the bone. I recommended excision. The man consulted another surgeon, who was of the same opinion as myself, and gave the same advice. Case 1 was by this time better; and I resolved to try carbolic acid. I gave him a syringe and a quantity of the solution, with instructions how to use it. Not hearing of him for weeks, I concluded that he had gone to some hospital to save further expense. However, about a month after he got the solution, I met him by chance, and, on examination, found the wound quite healed up, the bone being intact, and a very slight cicatrix on each side. Flexion was impaired, but extension was perfect.

*Case* 3.—Two bees stung me on the tip of the left ear. My pupil, being at hand, extracted both stings with tweezers. No sooner, however, had he done so, than the part became very painful, and before I reached the surgery—a distance of only thirty yards—the pain had extended all over the left side of my head as far as the mesial line. The pain was exquisite; and over the temple and mastoid process was most agonising. Thinking that undoubtedly nothing but a germ of some kind could produce such rapid results, I determined to try carbolic acid; and, having some solution ready (1 to 100), I had it injected by the hypodermic syringe into the orifices left by the stings. If the advent of the pain was rapid, its exodus was equally so. I had the feeling all that day, on the left side of the head, as if my hair had been sharply pulled; and the temperature of the left ear remained for the rest of the day much higher than that of the right.

It is, I think, very generally admitted that whitlow is caused by the absorption of disease-germs through the cuticle, either by the natural openings or by a solution of continuity. Undoubtedly this had been the case in Cases 1 and 2, and probably also in Case 3. Whether the orginal germ alone caused the mischief, is a point I must leave for others to decide. Case 3 may have been caused by a chemical agent; but the process of destruction did not go on until the antiseptic influence of carbolic acid was brought to bear upon it.

It does sound strange to say that a bone denuded of its periosteum, and in all probability dead, was restored to life, so to speak, by the application of an antiseptic agent. I can account for it in no other way than by supposing that the denudation was only partial, that vitality was kept up by the parts in actual contact, and that, on the destruction and displacement of the intevening foreign body, the parts again

healed and united, as we see in the healing of a sinus, the transplantation of teeth, or the more recent skin-grafting.

I would advocate, therefore, most strongly the use of carbolic acid as a dressing for unwholesome wounds, however hopeless the case may seem. By its use, pain is decreased, or altogether becomes *nil;* excessive granulations are arrested; unwholesome discharges are checked; bad odours are dispelled; the tendency to the destruction of tissue is replaced by a proclivity to heal; and the cicatrices are more sightly than those treated by the older methods—collodion and water-dressings perhaps alone excepted.—*British Medical Journal, April* 13, 1872, *p.* 392.

---

## SYPHILITIC DISEASES.

---

### 89.—CARBOLIC ACID IN GONORRHŒA.

By George Ashmead, Esq., Brierley Hill.

[Mr. Ashmead has been employing carbolic acid in combination with tannin and glycerine as an injection in gonorrhœa.]

The following has been the formula employed:—Carbolic acid, eight grains; tannic acid, eight grains; glycerine, half an ounce; water to one ounce. It appears to act as an antiseptic, arresting the discharge, and cutting short the disease. The following are two cases out of seven treated with carbolic acid.

A. B., aged twenty-two, sanguine temperament, presented himself with the urethral discharge, and complained of much smarting and scalding during micturition. Rest, abstinence from stimulants, and light diet were enjoined. This was, however, declared out of the question, as the following week was to be one of active exercise, and a more than a usual amount of exertion. Inability to avoid stimulants was also pleaded. The above injection was ordered to be used twice a day, and a mixture containing bicarbonate of potash, hyoscyamus, and decoction of broom prescribed. After the first day a spot or two were observable on the lint in the morning, and the painful symptoms disappeared; on the fourth day, considering himself cured, he neither took medicine nor injected; and on the morning of the fifth found a considerable discharge and some scalding. I saw him again on the ninth day, when he considered himself quite well; but, at my suggestion, continued the medicine and injection for a short time longer.

B.C., aged twenty, came with a gleet of some months' standing, for which he had used a variety of injections to little purpose. He was ordered to use the carbolic acid injection twice a day. After injecting a few times the gleet entirely disappeared,

but returned a fornight afterwards after a "big drink." This, however, soon vanished on resuming treatment. He keeps an injection by him "for fear it should return."—*Lancet, Dec.* 16, 1871, *p.* 874.

---

## 90.—THE IODIDES OF AMMONIUM AND SODIUM IN SYPHILIS.

By BERKELEY HILL, Esq., Surgeon to the University College Hospital.

It not unfrequently happens in cases where iodine is required, and the patient has taken iodide of potassium with good effect for a certain time, that this medicine either ceases to produce any result, or is no longer tolerated. *Faute de mieux*, mercury is sometimes resorted to; but, as the cases are not suitable for mercury, they generally become worse while taking it. In this difficulty, the iodides of sodium and ammonium are very useful, for they affect the body somewhat differently from the potassic salt. They can be borne by, and produce the effect of iodine in many persons who have become nauseated by the latter. They moreover contain less alkali per weight of the salt, and their alkali is less deteriorating to the blood than potash. One or two cases will illustrate my meaning. A gentleman was brought to me by his medical adviser, with extensive and obstinate syphilitic ulcers of the tongue and palate. He had had several other forms of syphilis during the nine or ten years he had been infected, and was in a weak and low-spirited condition. Early in the disease, he had taken mercury and other remedies in the usual way. Latterly, for the ulcers in his mouth, idodide of potassium had been given in doses gradually increasing to a scruple, and even half a drachm; but, when I was consulted, these had ceased to check the disease, and had produced iodism. In consequence of this, mercury and several other drugs had been tried again without benefit. Though his tongue was foul, his appetite, but for the pain of eating, was good. This is important; if the digestion be deranged, it must be set right again before iodine can have fair play. Under these circumstances, the patient was ordered to begin with eight-grain doses of iodide of ammonium in bitter infusion three times daily. He bore this well, and in the course of a few weeks his tongue and palate were nearly healed. For complete cicatrisation it was necessary to alter the form to iodide of sodium; and, during two years, he has been taking this salt at short intervals to prevent the relapse of his disease. Here is another case of syphilis appearing unconquerable by either mercury or iodine, which had been given by several medical men whom the patient had consulted. The case was interesting from the

probability of its being one of contagion through an infected monthly nurse "sucking the breasts." The child was infected at the mouth subsequently to its mother, and the history in the mother points to contagion having entered by the nipple. I did not see the patient until two years after infection; she had then severe ulceration of the tonguue and of the soft and hard palate, and limited necrosis of the bone in the palate. From the prescriptions which she showed me, she had been taking scruple doses of iodide of potassium, for some time with good effect; but, latterly, even double doses of her medicine had only produced severe iodism without controlling the disease. It was with some reluctance that she consented to try iodine again; but, after a few eight-grain doses of iodide of sodium, coupled with frequent washing of the mouth with hyposulphite of soda and glycerine lotion, her sufferings were so far removed that she announced, a week afterwards, that she could eat crusts with ease; and, in a month, except that a morsel of necrosed bone still adhered, the mouth was quite well. The course of this case since has been obstinate; gummata appearing elsewhere after that in the mouth had healed; but she still takes iodine, sometimes in one salt, sometimes in another, and occasionally mixed with Bell's liquid extract of sarsaparilla. It would be easy to multiply instances where, iodide of potassium having lost its effect, the progress towards recovery can still be maintained by another form of iodine; but that is needless.—*British Medical Journal, Dec.* 23, 1871, *p.* 725.

# MIDWIFERY,

## AND THE DISEASES OF WOMEN AND CHILDREN.

### 91.—ON THE PROPER MANAGEMENT OF TEDIOUS LABOURS.

By Dr. G. HAMILTON, Falkirk.

I have used for upwards of thirty years only the late Dr. Ziegler's straight forceps, and until lately only one pair of these. Shortly after Dr. Ziegler invented his instrument I called upon him in Edinburgh, and he recommended me to get the forceps which were then being made by Young, of Edinburgh. These consisted of the two blades which are now so well known, and also of a shorter blade, which Dr. Ziegler thought could occasionally be advantageously substituted for one of these. This forceps is thirteen inches long, and is a most admirable instrument. Its ingenuity, simplicity, and great utility, have hardly yet, I think, been justly appreciated by the profession, arising, perhaps, a good deal, in Scotland at least, from the circumstance that the brilliant reputation of Sir James Simpson, who shortly afterwards designed, and of course recommended, an instrument of his own, overshadowed and kept in the background this, in my opinion, the best of all forms of the forceps I have seen. My original instrument, after long use, I found to have certain defects, and on that account I have now laid it aside; but although I have done so, I look upon it with wonderful respect, as a servant that has been most useful to me in many trying situations.

The defects of this instrument I have found to be, that it was barely long enough in certain cases that have occurred to me, that it is not strong enough to be used as a lever in my mode of delivery or even with powerful traction, that the hinge has not sufficient "play" to allow the blades easily to lock, and that the open fenestra in blade No. 1 is apt, when much leverage is used, slightly to injure the cheek or side of the head of the child next the pubes. Mr. Young has constructed me two instruments to remove these defects.

Comparing these forceps, I would remark, 1st, that Ziegler's are longer than any of the others, and, I believe, also stronger. 2nd, That Sir J. Simpson's, which come nearest to them in length, have two projecting horns, nine and a half inches from

the extremeties of the blades, which must, I should think, limit their introduction to this length, whereas those wanting these horns may be easily locked even within the parts of the patient, if necessary. These appendages were added, no doubt, to give additional traction power, but this is amply secured by the great strength of those I am using. 3rd, Mine, inside the blades, measure two inches and five eighths, and the others one eighth, two eighths, and three eighths of an inch more. Now this, again, may become important in difficult cases, for, as Burns properly remarks (*Midwifery*, p. 503), "forceps whose blades could come considerably within three inches of each other may, in a particular degree of contraction, act better and require less exertion" than others. 4th, With a width of three inches between the blades the instrument may be simply a double hook or tractor, while with two and five eighth inches it may lay hold of the head, and enable us to use it and the head as a lever. 5th, Dr. Beaty well observes (*Braithwaite's Retrospect*, vol. li, p. 361), that the "elongate curve" (which is given by the lesser width between the blades of Ziegler's forceps) renders the introduction of forceps more easy. 6th, The solid blade will, I think, be found a great improvement, not only by preventing, as I have said, injury to the skin of the child, but also by facilitating their introduction; and, when smeared with india-rubber paste, making the large friction-surface that lays hold of the scalp really of considerable importance. In fact, the forceps when thus assisted in laying hold of the skin, become almost, as it were, an instrument combining the properties of the forceps with what was aimed at by Sir James Simpson in his air-tractor. In fine, I think my instrument, from wanting projecting parts, and thus being less liable than double-curved ones to injure the mother; from the ease with which it can be introduced and locked; from its great length, which allows us to reach all kinds of cases; and from its great strength, which enables us firmly to lay hold of, move, and mould the head, is superior to any other I have seen.

I have remarked that straight forceps can alone be used so as to gain the leverage power which I have indicated, and also that they must have their blades placed antero-posteriorly in relation to the pelvis. If we inspect the bony pelvis, I think we see the reason of this, as well as the advantages which are thus gained. The outlet of the pelvis has been described as an irregular oval space, with its long diameter placed from before backwards. I would rather incline to describe it as *two* irregular spaces, the posterior always oval and the anterior often so, running into each other at the point formed by a line drawn between the two spinous processes of the ischia. It is,

I think, in the anterior space that the head in my second and third classes mostly gets fixed, and we may, I think, perceive that mere traction must often have very great difficulty, or that it may be found impossible, with this alone, to move it, unless we either turn it round or throw it to a certain extent back (sometimes conjoining the two movements) into the posterior space. This is what I do in delivering in the way I have mentioned, as may easily be shown by performing the trifling but very striking experiment of placing the forceps in the bony pelvis, and making them describe under the symphysis pubis the semicircle, or more, I have alluded to. I have been called in by brother practitioners, using, as I did, Ziegler's forceps, who have yet been unable to deliver under these circumstances after hours of traction, where I have succeeded at once by altering the position and direction of the instrument—applying leverage, and thus throwing the head back into the posterior space. My friends, I dare say, have been apt to think that I possessed a superior instrument to what they used, but the true secret of my success consisted in the different mode in which I used it.

I can now, I think, with greater profit, say a few words as to how I came to use the instrument in the way I have described. But in truth I can hardly do this in a systematic manner, for I may say it grew up rather than was invented by me; and I confess I did not at first see, as I afterwards have done, the novelty and importance of the changes implied by it. Perhaps the first thing that particularly attracted my attention was the different position in these deliveries required to be occupied, in the bed, by the attendant. Formerly (the patient lying on her left side) she used to sit, say a little higher than the middle of the bed, gently elevating the patient's right knee. Now, I required that she should sit on the pillow, on *a line with the patient's head*, and I usually found that I had to make great exertions to get her to keep the patient's right knee and leg out of my way, I myself going into the bed and following the course taken by the forceps. I found myself always crying "Up, up, still further," till the nurse sometimes lost patience, saying she could do so no more. This, of course, arose from the much greater curve which I was now making the handles of the forceps take. This, in point of fact, is so great, that I often, as has been stated, find the handles of the instrument, at the termination of delivery, almost paralled with the abdominal parietes of the patient. In November, 1861, I delivered with the forceps a lady (primipara) lying in a bed which stood in the midst of the room, and I noticed that I introduced my forceps while sitting on the right hand side of the bed, that as the delivery progressed I went round the foot of the bed, that when

the delivery was completed I was standing at about the middle of the *left*-hand side of the bed, and that in the last stage of the delivery I had changed the position of my hands in grasping the handles, and was then *drawing* the instrument forwards and upwards. I did this, as I mentioned, with Ziegler's forceps; but had I been using such an instrument as I commenced practice with, it is manifest that the discovery of such an operation being possible never could have been made.

Let me now give a single case illustrative of these remarks, in which I used one of the new instruments made for me by Mr. Young.

Dec. 30th, 1870.—Was called about 1 p.m. to Mrs. G., who has a midwife with her, and states that she has been in labour since 7 a.m. Is of the squat figure I have referred to as having tedious or very difficult labours. States that she has had thirteen children (the first twins), of whom five were stillborn, three of these being delivered with the forceps. The head was at the brim of the pelvis, the uterus easily dilatable, and the os uteri about twice the size of a crown-piece. Pains regular, but rather feeble. On examination the pubes and promontory of the sacrum were found to project inwards, and between these and the head the uterus was caught. My first care was to get rid of this obstacle, which I succeeded in doing, first, by using two fingers in pushing up the uterus in front; and secondly, as this failed, by introducing my hand and extricating the engaged part of the uterus posteriorly. After a few endeavours of this kind the uterus slipped over the head, the latter immediately descending a little into the pelvis. The head was still, however, so high up that it was with difficulty the tip of one ear could be felt a little to the right of the symphysis pubis. The face, I thought, was to the right (which I afterwards ascertained to be the case), but of this I was not at the time quite certain. After the head had come a little further down into the pelvis I tried to apply my new forceps; and, by directing the handle of the blade No. 1 well backwards, I succeeded in placing it over the ear next the pubes. The other blade easily slipped into its place on the opposite side, and these gave me a remarkably firm hold of the head. I used simply traction at first, assisting each pain, until the perinæum began to be pressed upon, when I turned the instrument forwards and upwards (supporting the perinæum with my left hand), until the handles had revolved round the lower part of the pubes. When the head was born the instrument remained, as when applied, nearly in the mesial line of the woman's body. One blade was over the right ear and cheek, and the other over the opposite ear and parietal bone. There was a very slight excoriation over the left parietal bone; the anterior solid blade had merely compressed the skin and

made it look slightly grayish. The umbilical cord was round the neck, and the child for some time failed to breathe. By at once, however, applying my mouth to that of the child and inflating the lungs, and compressing the sides of the chest in conjunction with this (as I have recommended and described in the Edin. Med. Jour., for May, 1855), the child soon began to breathe and cry, and both it and the mother did well.

In my notes I find the following remarks on this case:—Here the length and strength of my new instrument gave me great advantage, for it must be remembered that in such labours strength is equivalent to length. I think my old instrument would have been constantly slipping until the head advanced, and hence losing most valuable time. And time here, as far as the child was concerned, was of immense importance, for, had the labour been prolonged even a very little more, the child must have been lost. From the time I saw this woman till her delivery something more than an hour elapsed. I should say that probably another half hour, and certainly another hour, in the passages would have killed the child. As it was, not an instant of time had to be lost in order to save it; and here I would remark that slapping the buttocks, dipping in cold or warm water, &c., would have been bad practice. In such extreme cases inflation, as the major remedy, should instantly be used, for then even seconds of time are precious. In delivery, the force applied was at first almost pure traction, and at last it became almost pure leverage. At first I tried to turn the head into the hollow of the sacrum; but finding this not to succeed, I let the instrument take its own course, the head passing the *os externum* with the blades nearly antero-posteriorly. Looking at it as a whole, I think we have here a key to the success which has attended my mode of practice, for it is easy to see how enormously a want of decision and knowledge must in these trying cases increase the mortality to both mother and child. Even though the ear was at the brim of the pelvis, and so high up that it could barely be touched, the simplicity of the whole operation, and the ease with which it was performed, seemed to me very remarkable. Once the anterior blade had been placed over the ear, I seemed to take almost no heed as to all the rest. The posterior blade at once took its proper place, and locked without giving me either trouble or thought (for this is the rare quality in Ziegler's forceps, that they lock of themselves); as the head advanced I did not know, and I did not care, what turn it would take, or whether it would take any, for my forceps were adapted to all contingencies; so, as it were, I almost let them take their own way, only watching the handles, which at once told me what was being done. If they should keep antero-

posterior, then leverage was the power I had mostly to trust to; if they became transverse, the curve they could go through, or the lateral oscillation that could be employed, I knew was very limited, and traction then would mostly be the power available. With such simple rules as our guides, it seemed to me almost impossible for a person of the most ordinary competency to go wrong.

But if, from timidity or other cause, I had allowed the uterus to be caught at the brim for an hour or two more, and as a consequence, the head to be arrested and the parts to become swollen, and had then introduced a curved instrument within the uterus, and had set out with a conviction that it must, and a determination that it should, describe a screw-like course in delivery, how very different might have been the results to mother and child. Looking at the difficulties of such cases, so managed, I think there can be little wonder that most writers on obstetrics have expressed a salutary dread of "long-forceps" deliveries, or that some should even have preferred to them opening the head.

The double curve appears to have been first given to the forceps by Levret, so that it might fit into the shape of the sacrum, and when the face has already got into the hollow of the sacrum, a short instrument of this kind suits very well. The curve has been retained on the longer instruments used since then on different grounds—as far as I can understand, because it gives a larger surface for laying hold of the head; because the head at the brim can be better reached with it; because, as Dr. Barnes says, it fits into the natural screw-like form of the pelvis. These and other reasons I have heard given for retaining the double curve, but I have never yet seen a case in which I would have preferred this to the straight instrument. In this I think the Dublin school, where Dr. Beaty's straight forceps are mostly used, is right. Dr. Barnes says he formerly used straight forceps, but has latterly laid them aside, in consequence of finding that they injured the perinæum. On this point I can only repeat what I have already stated, that I have never had in my *whole* practice rupture of the perinæum; and that, when using the forceps, if I find any unpleasant distension of it, I take them off, and support the parts with the palm of my *right* hand (not the *left*, as recommended by some authors), pushing the head strongly forwards and somewhat upwards, as recommended by the late Professor Hamilton, or in other words "shelling it out," as this manœuvre has been appropriately named by Dr. Barnes. As far, therefore, as I can judge, I would say that the instrument I now use is fit for the management of any case I have ever met with, and certainly it can be used in many where the double-curved forceps are wholly inadmissible.

My 4th class consists of such cases as cannot be reduced to the 1st, 2nd, or 3rd classes, which may arise from a variety of causes. To some of these I shall now advert ; and here I must repeat the opinion I have already expressed, that, with increased knowledge and experience, and the use of better instruments than we formerly had, we may reasonably hope the number of such will in future be considerably diminished. Let us take, for example, such a case as I have recorded in the Ed. Med. Jour., Oct., 1861, p. 320, where the ear was at the symphysis and the face to the left (my 5th position). In this case I could not get the face to rotate to the left, and was therefore obliged to push up the head, rotate the face to the right, and then deliver. If I met with such a case now I would, of course, at once endeavour to use the forceps as a lever in the manner described, and my impression is, that with my stronger instrument I should succeed. Supposing we fail, however, the principle of rotation, as I have pointed out in the article referred to, can be resorted to with great advantage, and to an extent (with straight forceps) that would astonish many practitioners. Sir James Simpson used to be much interested in this subject, and I recollect relating to him a still more interesting example of this kind. The case had been a long time under the care of an ignorant, and, what was even worse, an obstinate midwife, who persisted in the vain endeavour to make unaided nature effect the delivery. When I saw the woman I found the parts dreadfully swollen, and the case fast assuming all the characters of "impaction." With considerable exertion I found that the ear and face were to the left of the symphysis (my 4th position), and succeeded in fixing the forceps over this ear and the head, but could neither turn the face into the hollow of the sacrum nor make it advance downwards. I then, as in the last case referred to, pushed the head above the brim, and rotated it so as to place an ear and the face to the right of the symphysis (my 1st position), and still I could not deliver. I then resorted to podalic version, and by that means saved the child. The mother, however, succumbed to peritonitis in three or four days afterwards. Besides showing the extent to which rotation can easily be carried, this case is one among many others confirming the doctrine that the base of the child's head, when brought down first, will sometimes pass easier through a narrow pelvis than if the vertex had presented. With my later ideas as to the mechanism of labour, and the powers of the forceps, I would give my adhesion to this great principle with reservation ; and I would say as the result, of my own experience, that podalic version should never be resorted to until the forceps (with rotation to the right, should the face be to the left) have failed.

When I am absolutely compelled to apply the forceps within

the uterus, the rule I follow is still, if I can, to place them over the ears, which mostly requires that the anterior blade (No. 1) should be thrown well back on the perinæum; or, if I cannot do this, or cannot exactly ascertain where the ear lies, I then lay hold of the head wherever I can (sometimes placing blade No. 1 posteriorly before introducing No. 2), and advance it until, by examination, I find its exact position, when I then know precisely how to proceed. At the same time that the forceps draw the head through the os uteri, I find it most useful, with the fingers of the left hand, to push the uterus up over the head. In these high operations the length and strength of my present instrument give me, I think, very great advantages.

Cases with the face to the pubes, occurring in primiparæ, have given me some trouble. My 732nd case, as I have stated, was of this kind, and I lost the child, and in an earlier part of my practice I recollect the same thing happening in another instance. It was interesting to me, therefore, since I have been using my new instrument, to meet with a case exactly similar. It was under the care of a midwife, who called me to assist her. I found the head jammed in the parts very tightly, and, as I could not at first ascertain with precision the position, I placed the blades antero-posteriorly, and used as much leverage as the case would allow. In doing so I suddenly felt the handles jerk round and become transverse. Taking the instrument off and examining, I now found that an ear was slightly to the left of the symphysis, with the face to that side also. Reapplying the instrument I again used leverage, when the head made another jerk round, placing the face in the hollow of the sacrum, and delivery took place immediately. Here, again, I was much impressed with the great power my new instrument and mode of delivery gave me, and I was much pleased with the firm hold the india-rubber on the inside of the blades (which I had lately been using) seemed to give. The simplicity of the operation especially struck me, for it was nature, and not I, that took the turns. I found, on examination, that the solid blade (No. 1) had at first been placed over the left eyelid and cheek, which were a little red and swollen, but not in the least degree excoriated. This disappeared in a few days, and mother and child did excellently.

After all, of course it will occur that in some instances the disproportion between head and pelvis is so great that delivery cannot be effected with the forceps, though I am now inclined to think, if the case has been well managed, and absolute deformity of pelvis or hydrocephalus be absent, this is much rarer than is generally imagined. In such cases, if the mother has not previously been too much exhausted, podalic version

naturally presents itself to us as another resource before perforation. From the line of practice which I have followed, I have had so much less experience in podalic version than most hospital accoucheurs, that I cannot venture to speak practically on the subject with great confidence. I would remark, however, that in such cases, where the head is very large and refuses after version to enter the pelvis, or where it is caught high up and does not advance after prolonged traction, it is perhaps the best practice, if the want of beat in the funis indicates the child's death, at once to use the perforator. In the case immediately preceding my series of 731 cases I did this, the child being almost a monstrosity in point of size. In another case, where I had to assist two brother practitioners, I did the same, the cervical vertebræ having given way before it was resorted to. In the first case the mother died also, from peritonitis, while, in another case still, I was called by two practitioners to assist them, and found the head alone in utero, and the mother moribund. Such examples, conjoined with the mortality to the children in cases under my own care, already referred to, have, I confess, given me a great dread of turning and high footling cases.—*British and Foreign Medico Chirurgical Review, Jan.* 1872, *p.* 171.

---

## 92.—ON PUERPERAL FEVER.

By Dr. Edward Martin, Professor of Medical Midwifery in the University of Berlin.

[In the year 1860, Professor Martin first put forward the theory that puerperal fever depended upon a diphtheritic process set up in the female genital organs. The views of this disease held by the profession are extremely divergent owing to all the febrile diseases of lying-in women being included under one rubric.)

Febrile conditions may be met with in lying-in women as well as in non-pregnant women, whether as a consequence of inflammation in almost any organ (but which has no connexion with the puerperal condition) or in connexion with various contagious diseases, as scarlatina, variola, &c. There may even be febrile affections consequent upon inflammatory action in the genitals of lying-in women, but which are essentially different from puerperal fever in the alarming sense of the word. Entirely unconnected with this, lying-in women may have very severe fever from inflammation of the breasts or nipples, after contusion or laceration of the uterus or vagina, as well as consequent on abscesses or ulceration which may ensue upon effusion of blood into the connective tissue. Such

fever neither in its course, symptoms, or issue, resembles the conditions which arise from ths diphtheritic process ; and it is this which should be regarded as the essential characteristic of puerperal fever. Even the existence of thrombosis is not as a matter of course to be attributed to puerperal fever, as in many cases this remains entirely isolated, as contrasted with the thrombo-phlebitis which accompanies or follows the diphtheritic process.

Limiting in this way the conception of puerperal fever, the question naturally arises—What are we to understand by the "diphtheritic process?" You are aware that recent investigations have thrown most important light on the nature of diphtheria affecting other organs—especially the pharynx, where it has been shown to consist of a fungous formation, the spores of which are seen under the microscope to penetrate not only into the tissues, but within the bloodvessels—producing in this way a generalised disease. In diphtheria of the genital organs investigations have as yet not been extended thus far, and it remains a question calling for farther examination. Admitting, however, that the diphtheria is here due to a fungous formation, other questions arise. Is the fungus in question specifically different?—since we are familiar with various fungi which germinate in the vagina of both pregnant and non-pregnant women without giving rise to any dangerous affections;—is the fungus the mere carrier of the contagium? or is the puerperal fever produced in consequence of the special condition of lying-in women favouring the production of certain fungi, by reason of changes taking place in the organic substances and fluids?

Leaving these considerations, we may next advert to an examination of what the microscopico-anatomical basis of puerperal fever is. In the majority of cases we find on the external genitals and the vagina a diphtheritic deposit covering those wounded spots which, in the form of larger or smaller lacerations of the mucous membrane, so frequently occur during labour. The circumference of these spots is more or less considerably swollen. In many cases the diphtheritic deposit is thus confined to the external genitals, and the disease pursues its course by casting off the deposit without any or with very little general disturbance. But in the majority of cases coming under medical recognition, the diphtheritis is not confined to the entrance of the vagina, but is found deep within the canal, covering the large or small lacerations of the os uteri, and within the cavity of the uterus itself. Here it occupies both the site of the placenta and the upper parietes of the organ; and it is sometimes found exclusively here, and in no places accessible to the eye.

It may be objected that in many autopsies of women dying of puerperal fever no diphtheritic deposit has been found. This is a fact which I have myself verified in several instances, in which not only have the symptoms been present, but careful examination of the patient during life has shown the presence of the deposit. In explanation of this apparent contradiction, we must not forget that the diphtheritic deposit in many cases very quickly disappears, and especially when injections or caustics have been employed, while its consequences may persist and undergo farther development. That we should not be able during life to see the diphtheritic deposit when within the uterus is conceivable enough, but the diphtheritic flocculi may be recognised in their expulsion with the returning uterine injections.

As a general rule, the diphtheritic process spreads rapidly from the genital organs, but it does so only rarely towards the skin of the thigh, nates, &c. These then exhibit an erythema (which has been well named puerperal scarlatina) or pass into ulceration. More frequently the diphtheria extends into the urethra or the rectum, if it have not already appeared there primarily; but its most common modes of spreading are either by means of the connective tissue surrounding the vagina and neck of the uterus, by the mucous membrane of the tubes to the peritoneum, or by the lymphatics and veins—these various modes of its extension being often combined with each other.

In various but frequently combined modes of extension of the diphtheritic process of the genital organs, the great glandular organs of the abdomen, the kidneys, liver, and spleen are soon implicated, so that they are usually met with in a state of parenchymatous inflammation; and finally, the lungs, especially at their lower lobes, not infrequently exhibit the turbid-serous infiltration, pleuritic exudations being also associated with the peritoneal. A more infrequent result of the diphtheritic process, because in general a longer duration of the affection is required for its production, is inflammation of the peripheric cellular tissue, which may happen in different parts of the body. This most frequently occurs in and around the joints, around the muscles of the extremities (e.g., in the pernicious form of phlegmasia dolens), or around some of the superficially placed glands, as the breast or parotid.

It is precisely this great multiplicity of local affections, and their combination with each other, that constitute the peculiar characteristic of puerperal fever. As, however, sometimes one and sometimes another of these occupies the foreground, we are furnished with the explanation of why different authors have come to regard these different local affections, whether peritonitis, phlebitis, lymphangioitis, phlegmon, &c., as the essential feature of puerperal fever.

Although, in regard to our knowledge of the etiology of puerperal fever, decided progress has been made in recent times, yet many points remain obscure. Thus, in relation to the admission that the disease is autochthonous—i.e., that it may arise from the spontaneous decomposition of retained portions of the placenta—we must not overlook the fact, that remains of the placenta or membranes are not unfrequently retained for days, weeks, or months within the genitals without any putrid decomposition taking place, or any symptoms of puerperal fever appearing, while their presence often gives rise to repeated attacks of hemorrhage. If, then, in numerous other cases the retention of such remains is followed by septic decomposition and puerperal fever, it is evident that some other circumstance has to be sought for which has determined this unfortunate occurrence. From the known influence of the air in exciting putrefaction in fermentable bodies, it results that decomposition of the retained remains of the placenta would be especially expected when these protruded from the os uteri into the vagina, while they would be more protected from the influence of the air when enclosed within the cavity of the uterus. And, in fact, in this last case putrefaction does much more rarely occur; but it must not be overlooked that the remains of the placenta are then more intimately united with the wall of the uterus. However, there are plenty of examples of the occurrence of puerperal fever, notwithstanding complete expulsion of the placenta; and in such cases we must seek for other causes. Numerous cases have proved to me that women who are delivered while the subjects of recent gonorrhœa frequently become affected with puerperal fever, the diphtheritic process being immediately set up, and proving difficult of arrest. I must therefore admit that a preceding inflammatory condition of the mucous membrane of the genital organs stands in a certain relation to the occurrence of the diphtheritis. In the great majority of cases, however, the germ of puerperal fever gains access in other manners; and this is very positively shown by the well-known fact (confirmed by the numerous figures of the Vienna Lying-in Hospital, as also by the results observed in my own clinic, that the so-called street-births (*Gassengeburten*) are scarcely ever followed by puerperal fever. The transport of the diphtheritic germs takes place beyond all doubt very frequently during labour, more rarely after delivery, and sometimes shortly prior to parturition. In what the transported germ consists is less made out. Experience has taught us that cadaveric products and decomposed animal substances place puerperal women in danger, especially when an internal examination is made by fingers that have had to do with dead bodies without having been afterwards cleansed—although practi-

tioners may also convey the disease who have observed care in washing. The dead bodies in question have not always been those of the subjects of puerperal fever, although these entail a greater degree of danger. Again, certain secretions from suppurating wounds and ulcers conveyed to the genitals of a puerperal woman may give rise to diphtheritis. The epidemic prevalence of puerperal fever in Berlin during the winter of 1870-71 may with strong probability be attributed to the employment of so many of the civil practitioners in the military hospitals. Still more decidedly are diphtheritic products —which not infrequently are produced in scarlatina, typhus, cholera, suppurating cancer, &c.—dangerous to lying-in women. The most usual mode of propagating the diphtheritic poison from the sick to the healthy is its direct conveyance by means of sponges, dirty towels, catheters, clyster-pipes, or the fingers of the accoucheurs; and in this way epidemics of puerperal fever are brought about most frequently in hospitals, although they are also met with in private practice. What relation this origin bears to an incubation stage is uncertain; for although Veit has observed this to vary between twenty-seven and forty-eight hours, the number of cases adduced are as yet too few to allow of any general statement being made.

Diphtheritis of the genitals is not only met with in puerperal women, although they—on account of the denudation of the mucous membrane of its epithelium, and the numerous lacerations of tissue, as well as the ready decomposition of the lochial secretion—exhibit a special predisposition for contracting the disease, while the dilated vessels present a favourable condition for generalising the affection. Paul Dubois, forty years since, observed that the pupils at the Maternité, who, while menstruating, tended sick puerperal women, also became the subjects of an affection resembling puerperal fever. In Germany similar observations have been published, showing that, under certain favouring circumstances, a similar diseased process may be set up in non-pregnant women. I remember the case of a woman 52 years of age, who was admitted into the gynæcological clinic of the Berlin Charité on account of repeated hemorrhage. This arose from a large crumbling myoma, for the removal of which I used a forceps which probably had not been properly cleansed after a former employment. The woman died of diphtheritis of the internal genitals fifteen days after the operation. The autopsy disclosed the same lesions as are found in women who have died of puerperal fever—viz., diphtheritic deposit upon the wounded surface whence the tumour had been removed, lymphatic vessels filled with pus, and peritoneal exudation. It would seem to result, from other cases, that this diphtheritic process of the genital organs in non-

pregnant women is but rarely followed by dangerous general disease.

With regard to the prognosis of puerperal fever in general, if we except the cases in which the diphtheritis remains localised, it is upon the whole unfavourable; for we must admit that one-third of the cases in which fever has ensued upon diphtheritis of the genitals terminate fatally. Death takes place most frequently up to the fifth day, and then up to the eleventh day. In some cases the disease may last even for months.

I have only a few words to say concerning treatment. The prophylaxis lays claim to our most earnest attention, and the etiology of the disease indicates many important points for the exercise of this. The extremest cleanliness of all having to render service to the lying-in woman, both as regards their person and their clothes—especially their fingers and sleeves—and cleanliness in regard to all clothing, catheters, sponges, enema-pipes, &c., must be most stringently insisted upon. It is very much to be desired that all the utensils of labour should be new for each woman, and the same elastic catheters should never be employed for several lying-in women. As mere washing the hands which have become contaminated with infectious matter does not seem to afford sufficient security for internal exploration, I think it best under such circumstances to rest satisfied with external exploration. Especially does this rule apply to lying-in hospitals when cases of diphtheritis have appeared; and my own experience on this point entirely confirms the propriety of the advice given by Halbertsma and Litzmann. How necessary, then, is that complete practice of external exploration which I have taught since I first held the Professor's chair, speaks for itself. Lastly, in regard to the curative treatment. It must be pre-eminently symptomatic, and, as long as the temperature continues high, before all things the fever should be diminished. Internally, digitalis with nitre or acids, and externally tepid or cold applications, contribute to this end, after due evacuation of the bowels has been secured. I cannot speak so well of quinine as do many authors. Local treatment has during the last ten years rightly been much tried. Cleansing out the vagina by syringing and injections of tar- or creasote-water, with carbolic acid, chlorine, or solution of nitrate of silver, has without doubt proved of great utility, even although it has not often happened that the process has been cut short by their agency. Injecting the same substances, suitably warmed, through a large catheter *à double courant* into the cavity of the uterus has sometimes been followed by a considerable diminution of temperature, as well as cleansing out the uterine cavity; but a decided general

improvement has been by no means of such frequent occurrence as might have been hoped.—*Medical Times and Gazette, Nov.* 11, 1871, *p.* 583.

---

## 93.—CARBOLIC ACID AS AN ANTI-ZYMOTIC IN PUERPERAL DISEASES.

By Dr. J. D. Trask, Astoria, United States.

[The poisonous effects of carbolic acid when given internally appear to have prevented the proper trial of it for the purpose of arresting the progress of blood contamination. In this case no ill effects of any kind exhibited themselves.]

On the morning of the 25th she was seized with a rigor, which was represented as of very great severity, followed by intensely high febrile action, alternating through the day with oft-recurring chills of short duration. I was prevented from seeing her until four o'clock in the afternoon. At that time there were great heat, profuse sweating, pulse 160, extreme restlessness, and increased tenderness over the region of the uterus, and in the right iliac fossa. There had been urgent diarrhœa, which yet continued, the dejections being involuntary and of intolerable odour. The lochia were suppressed. There was not only no nausea, but a craving for food. Taking into consideration her experience before labour set in, I could not but feel that there was a positive septic condition of the system in connection with symptoms of peritonitis, and it occurred to me to attempt to impregnate the system as far as possible with carbolic acid. Its administration by the mouth, rectum, and vagina was at once commenced. A half-drop of Calvert's solution was given in mucilage by the mouth every two hours. The solution for the rectum was one drop increased to five drops to an ounce of mucilage, a half-gill being thrown up after every dejection. The vaginal injection was not less than five drops to the ounce every three or four hours; the vagina, in my experience, being more tolerant than the rectum of the first impression of the acid. There was at the time a supposed idiosyncrasy forbidding the use of opium.

Within twenty-four hours the diarrhœa ceased. The pulse was reduced to 120. There was still exquisite tenderness over the uterus, and great thirst. The inflammatory symptoms were henceforth in the ascendant, and the signs of septicæmia, although recognised in different degrees of distinctness, were in abeyance. The system seemed to be saturated with carbolic acid. She tasted it in every thing, inhaled it, as she thought, in every breath (I cannot say that she *exhaled* it), and was so disgusted that she refused to have it longer brought near the bed for any purpose. No other antiseptic was substituted by

the mouth, but the bisulphate of lime or the permanganate of potash was employed abundantly in vaginal injections until convalescence began.

By the 29th tenderness had almost disappeared, but the skin was still hot and dry; there was delirium at night; the urine scanty; still great craving for food; involution of the uterus apparently not begun.

June 1. Her aspect was much worse. There was dorsal decubitus with knees drawn up, area of tenderness increased, irritability of the bladder and rectum, some diarrhœa, and that offensive, and a great deal of tympanitis. There was urgent desire for food, and constant tendency to reject it in small quantities from the mouth. The pulse was 140; respiration, 40; temperature, 106·5°. A large blister was applied to the abdomen, and its beneficial effects in reducing tenderness were very marked: the pulse fell to 120. It became necessary to withdraw the urine by the catheter.

June 2. Countenance in repose haggard and anxious; respiration 40; the pulse 128, a mere thread; surface covered with cool moisture from head to foot. The process of involution had made no perceptible advance; the lochia consisted chiefly of a light-coloured, sanious mucus; the tongue red and tremulous; the urine now copious.

Dr. B. Fordyce Barker saw her on this day, and expressed the conviction that within three or four days she would sink. This was based upon a consideration of the state of the pulse, and the evidences of metritis, peritonitis, and septicæmia, in the light of cases of a similar character which he had seen.

She was at this time taking beef-tea and milk-punch freely. It was found on trial of the aqueous tincture of opium with bromide of potassium, and afterwards of opium alone, that she could take it advantageously for relief of the irritation of the bladder and occasional diarrhœa. The urine examined at this date was found to abound in granular casts, and to be highly albuminous. She could not be convinced that she was seriously sick, much less that she might not recover, but was determined at any rate to get well.

By the 5th the urine deposited blood corpuscles in large quantities, but there was much less albumen. The uterus was sensibly diminished in size.

June 7. For three mornings there has been a slight rigour, followed by a degree of reaction; the lochia profuse, and very offensive when antiseptics are intermitted; is constantly hungry, but can take only fluid nourishment, and in small quantities; pulse, 120; respiration, 20; temperature, 99°.

June 8. Increased fever; pulse, 160; urine of the 7th abounds in pus and epithelium, with a few casts.

June 12. Surface cool to touch and moist; temperature 101·5°; involution sensibly progressing; tympanitis continues.

June 18. Tenderness only on firm pressure; uterus not to be felt above the pubes.

June 27. Cystitis renewed with great severity; the exciting cause was evidently the misconduct, for two or three days, of the wet-nurse. Mental excitement always produces irritability of her bladder in health.

July 7. She got an attack of pleuritis with effusion.

July 9. For four days there had been headache and absolute loathing of food. She woke on the 9th, crying out for something to eat, and found that her menstrual flow had come on.

July 21. The suffering from cystitis has been protracted and severe; the urine most of the time being very turbid with pus and mucus, and constantly alkaline. She is at present able to sit up for a half-hour, and but for the cystitis her convalescence would have been much more advanced. To-day I find the uterus much enlarged, anteverted, and fixed in its position by lymph thrown out anteriorly, posteriorly, and laterally, during the progress of peritonitis in the pelvis. The roof of the pelvis is a plane of induration, with the uterus implanted in the centre. In a recumbent position she suffers from uterine tenesmus, and from a sense of falling of the viscera of the pelvis in standing up.

August 7. Enjoyed a ride in a carriage.

In this meagre abstract of the notes of this case it will be seen that pelvic peritonitis probably existed to a certain degree before labour; after being confined to the pelvis for several days it invaded additional portions of the peritoneum through a considerable portion of the abdomen. But, apart from the symptoms due to peritonitis, there was throughout the case another class that could be attributed only to blood-poisoning. The profuse and highly-offensive diarrhœa, which occurred unconsciously to the patient, accompanying the violent rigours, cannot well be explained on other grounds than the introduction into the system of a septic agent, which produced this great disturbing influence. Later in the case, when the peritonitis had subsided, the influence of this subtle agent could be recognised in the slight rigour and exacerbation of almost daily occurrence, and in a very peculiar odour of the cutaneous secretions, which was so persistent as to attach itself to the fingers for hours.

If any consider the symptoms related as evidences of nothing more than puerperal peritonitis, I refer to the fact that my opinion was confirmed by Dr. Barker, whose opportunities for studying puerperal diseases have been rarely excelled by those of any observer.

Now, if it be asked of what use was the carbolic acid if the septicæmia continued in *any* degree after its employment ceased, I would suggest that it is not unreasonable to assume that the first charge of poison which the blood received was neutralized by the carbolic acid, and that by the persistent subsequent employment of antiseptic injections any further impregnation of the system in any considerable degree was prevented. During the twenty-four hours of its administration six drops were taken into the stomach and at least sixty drops brought in contact with the vaginal and rectal mucous membranes; doubtless no inconsiderable portion of the latter was absorbed into the circulation.

The readiness with which carbolic acid may be taken up by mucous membranes was first suggested to me by the fact that a patient, to whose endometrium it had been applied, spoke almost immediately afterwards of tasting the carbolic acid in the mouth. In using it in the present instance as an injection into the rectum, apart from its antiseptic influence, I had much confidence in its ability to moderate the diarrhœa by its sedative influence. The anæsthetic influence of a weak solution in burns is now pretty well known to the profession. Its value for allaying rectal tenesmus is not, I apprehend, so generally appreciated. In irritations about the anus, combined with glycerine and a mineral astringent, it is invaluable, and in dysentery I have fonnd it capable of affording great relief when dissolved in mucilage. The susceptibility to its use varies in different persons. When thrown into the rectum, I generally commence with a solution of one drop of Calvert's solution to the ounce, increasing the strength if necessary, until a feeling of decided warmth is produced by its administration, and this is followed by local anæsthesia.—*New York Medical Journal, Oct.* 1871, *p.* 357.

---

## 94.—REMARKS ON UTERINE FIBROID TUMOURS AND POLYPI; THEIR PATHOLOGY AND REMOVAL.

By Dr. Thomas Skinner, Physician to the Liverpool Lying-in Hospital and Ladies' Charity.

[There is no doubt that the more immediate the removal of a pediculated fibroid, or fibrous, or any kind of pediculated tumour within the uterus, the better; consequently, those who have had the largest experience in the removal of such tumours never resort to the ligature. The modes of operating may be briefly stated as follows:]

1. *Preliminary Steps.*—In order to facilitate the removal, the first step to be taken, is to open up the passages for the intro-

duction of the necessary instruments, and to obtain greater certainty as to the position, relations, and attachment of the tumour and its pedicle. This, we are all aware, is best obtained by the introduction of sea tangle and sponge tents, and, if necessary, by incising the cervix uteri.

2. *Immediate Steps.*—As a rule, the fixing of the tumour by means of a volsella, or by means of Dr. McLintock's corkscrew, which, though common-place, is a most efficient instrument, and generally easy of application; transfixing with a cord, or placing a noose over it is next required. As regards volsellæ, there is no instrument in surgery worse made; simply because they are almost always made too sharp in the prongs. The prongs are generally made round, small and sharp—whereas, they ought to be square-shaped, tapering and sharp, combined with strength in all their bearings. As a rule, they are too long also between the joint and the prongs. The small and sharp-pronged volsellæ do not keep their grip; as soon as traction is made use of, they tear their way out; while the square-shaped ones keep their hold like bull dogs. The tumour, being firmly grasped, is pulled as low as it will admit of, and held there, whilst an ecraseur is passed over it, which may be accomplished in various ways. A noose may be made with the wire or chain and passed over the handles of the volsella, and ultimately over the tumour up to the pedicle, which latter may then and there be severed. Sometimes, the pedicle is so thick and tough that the division is not so easy as *à priori* we might be disposed to imagine; as in the removal of one specimen where the pedicle was two inches in diameter, and fibrous throughout. Such being the case, our next best step is to divide the pedicle immediately above the wire or chain of the ecraseur, with blunt-pointed curved scissors; or with a blunt-pointed curved bistoury, protected with lint where the cutting surface is not required; or, best of all, with the ingenious polyptome of the late Sir James Simpson. Two tumours were severed from their attachment to the uterus in a few seconds by means of this instrument—an instrument positively harmless to the surrounding parts. The best form of intra-uterine scissors for the purpose are the invention of my esteemed colleague, Dr. Grimsdale, of whose manipulative dexterity and surgical skill in the removal of such tumours it would be impossible to speak too highly.

Some of these pediculated tumours admit of being removed by traction with the fingers. I may instance one case of a patient of Dr. Swinden's, of Wavertree: a lady who had flooded more or less for three weeks after her confinement, and who was *in articulo mortis*, when I saw her with Dr. Swinden. A pediculated fibroid, the size of a pigeon's egg, was attached to

the fundus uteri. By gradually dilating the parts, I got my hand in the vagina, and my fore and middle fingers into the uterus. By passing a finger on either side of the tumour, and finding that the pedicle was small and yielding, I pulled it off with a twisting motion. All hemorrhage ceased; and the patient, from the last stage of anæmia, made a perfect recovery. In another case of a similar kind, which I lately saw with Dr. Swinden and Dr. Le Gross, of Wavertree, I removed a small fibrous or warty growth, along with a few mucous polypi, after the passage of sponge tents, by means of the nail of my right middle finger. In this instance the patient was quite as anæmic as the last. All hemorrhage ceased with the removal of these trifling offenders. As an aid in the removal of such polypi, and of rugous and hemorrhagic conditions of the lining membrane of the uterus, the uterine scrapers of Recamier, Simpson, and Locock are most useful.

There is still one other class of uterine tumours admitting of immediated removal, though not pediculated. They have no name other than fibroid polypi; they are really intra-mural tumours of the sessile or submucous variety, but they are small, almost always single, generally limited to the cervix-uteri, and not infrequent in their occurrence. They are generally of an ovoid, almond, or walnut shape, and one extremity generally projects into the cavity of the cervix or presents at the os uteri. I have seen several of this variety, and I have assisted the late Sir James Simpson in their removal. I have already alluded to them, and their mode of removal, in the first or pathological part of this article. I shall only repeat that, if *come-at-able*, they are perfectly safe to remove by Simpson's polyptome, and torsion, or by crushing with a pair of forceps I got made for that purpose and for simplifying and shortening the operation of craniotomy. It would appear that the life is easily *crushed out* of these growths; the curious part of it being, that, although they themselves are easily destroyed, it is sometimes very difficult to get at them to kill them, without endangering the life of the patient. This leads me to allude to another method of removing this growth; and I commence doing so by asking a question of myself. Is it wise to partially remove a fibroid polypus? Undoubtedly it is; and the more we can remove the less there will be, as a rule, of hemorrhage thereafter. I remember a case, in which Dr. Grimsdale assisted me, at Waterloo. We broke two strong wires, and at last succeeded in passing a strong chain ecraseur over all that was *come-at-able*—about one half of the substance of the tumour. We removed this mass; and the patient, who, for ten or twelve years before had flooded at each menstrual period, never more lost a drop of blood. Previous to the operation, a tumour of

the size of a cricket ball was felt in the hypogastric region. On examination of the patient, about eight or nine months afterwards, there was no trace of any tumour whatever; nothing ever came away at all resembling a solid substance. This case was the first to lead me to the conclusion that it is sound practice, in severe hemorrhagic cases, to remove as much of the tumour as is possible; and, if it cannot be cut with the polyptome or by the ecraseur, it may be crushed out of existence, or by a combination of crushing and cutting. As regards the justice and safety of this line of treatment. I know that it was practised by Sir James Simpson, by Mr. Baker Brown, in his gouging process, that it has been practised by McLintock, of Dublin, is patronised to a certain extent by Dr. Matthews Duncan, and that it has forced itself upon various practitioners, as recorded in our journals. One of the most instructive examples will be found in the Transactions of the Obstetrical Society of London, vol. x., where Dr. Hall Davis removed part of a large intra-uterine fibroid tumour, $6\frac{1}{2}$ ounces, then excised in pieces 3 ounces, and ultimately a mass, which was thrown off entire, weighing $8\frac{1}{2}$ ounces; in all, the tumour weighed 18 ounces. The lady made a perfect recovery. Such a case is very encouraging; but, as our American cousins would say, "the conservative drag after all is a valuable institution."

Regarding one tumour, which weighed half-a-pound, I must say a word or two. The late owner of it consulted me on account of a draining menorrhagia three years ago. As the complaint resisted the usual and best remedial measures, I surpected organic mischief. On examination I detected a small intra-uterine polypus. I told the lady there was nothing for it but a surgical operation. I saw no more of her for three years, two of which she had spent under the care of a homœopathic physician in London, who told her that a surgical operation was quite unnecessary, and that he would cause the tumour to be *absorbed*. Of course she believed this, even although the monthly and fortnightly flooding went on the same. After being two years treated thus, circumstances forced her back to Liverpool, and she received advice, first from one and then from another homœopath, for another year, but all to no purpose. As the patient's life was ebbing, and all but *in articulo mortis*, she and her friends begged of me to take her case in hand. I took counsel with my friend Dr. Grimsdale, and we determined at once on removal of the tumour, which was executed in about twenty minutes, by means of Braxton Hicks's ecraseur, and Simpson's polyptome. From the time of the operation until now, nearly nine months, she has never lost one drop of blood, or ever "looked over her shoulder." The result of three years of homœopathic absorption by means of

specifics was, that the tumour *grew* from about half-an-ounce to eight ounces.

In conclusion, I shall enumerate the necessary *armamentaria* in the removal of fibroid tumours and polypi. The uterine sound or probe is most useful in diagnosis, and for measuring the probable size and even weight of the tumour. From the length of the cavity, one can frequently guess correctly the weight of the tumour. The wire ecraseur of Dr. Braxton Hicks, and a chain one also; Simpson's volsellæ, large and small, and his polyptome; Grimsdale's intra-uterine scissors; strong, blunt-pointed curved scissors, for incising the lips of the cervix-uteri, if necessary; a small uterine syringe, for hœmostatic injections; one or more uterine scrapers; my own polypus crusher, and a small and narrow pair of midwifery forceps, with sponge and laminaria tents, and their guides or introducers; wire and other ligaturing material; sponges, and an ordinary pocket case of instruments, may all be required in the removal of a single fibroid tumour or polypus. anywhere within the os uteri. In some few instances, additional instruments are required; those mentioned, however, will generally meet every requirement, especially where the operator is up to his work. I have not mentioned the vaginal speculum, because in tumours of the kind treated of, it is rarely of any use.—*Liverpool Medical and Surgical Reports, Oct.*, 1871, *p.* 1.

---

## 95.—ON THE TREATMENT OF FIBROID TUMOURS OF THE UTERUS.

By Dr. Alfred Meadows, Physician-Accoucheur to and Lecturer on Midwifery and the Diseases of Women and Children at St. Mary's Hospital, &c.

It is generally admitted, I believe, that morbid growths partake more or less of the character of the tissues in which they are developed. Hence bones develope bony tumours; fat, fatty tumours; nerve, neuromata; cartilage, enchondromata; and so forth. Those which we are considering are no exceptions. Examined microscopically, fibroid tumours of the uterus present almost the same histological characters as the uterus itself. There may be slight variations in the relative proportions of the several elements, as compared with the proper tissue of the uterus, and different tumours will present different proportions of these; but in the main, there are the same structures, viz., smooth or unstriped muscular fibres, bound together with varying quantities of connective tissue; sometimes one, and sometimes another, of these elements will predominate.

Now, it is thought by some writers that, such being the composition of these tumours, there is nothing remarkable in their gradual absorption and disappearance, any more than there is in the diminution of the uterus after delivery, as in the ordinary process of involution. It is implied, in fact, that the molecular changes are the same in both cases, and surprise is expressed that any one should entertain a doubt on the subject. The fact remains, however, that the great majority of observers do entertain very serious doubts on this question; or perhaps I ought rather to say that they have no doubt at all about it, but are decidedly of opinion that such changes, if they ever happen, are of extremely rare occurrence. I have myself seen many scores of these cases, but I have never yet met with one in which I was able to satisfy myself that any appreciable diminution of the tumour was effected either naturally or as a result of medical treatment. It seems to me, moreover, that the reputed explanation, if it have any foundation in fact, should be capable of almost constant demonstration; that, in short, the cure of fibroid tumours of the uterus ought, if this hypothesis be correct, to be the rule rather than the exception; for we all know that we have almost complete control over the process of involution; and that, if it be delayed after delivery, we can easily stimulate it into action. Why, then, can we not secure this result in the case of these morbid growths?

In regard to pain, these tumours are not, as a general rule, what would be called painful, unless their size be such as to cause pressure on neighbouring parts, or unless they be so placed as to project prominently from the peritoneal surface. A patient may have a very large tumour growing into the uterine cavity with little or no pain, but a tumour even of small size growing from the peritoneal surface will often occasion very great suffering. I have generally noticed that pain and hemorrhage are in inverse proportion the one to the other; and just as, if there be much pain, the tumour will most likely prove to be subperitoneal, so, if hemorrhage be the prominent feature, the tumour will in all probability be either interestitial —that is, in the wall of the uterus, or, as more often happens, it is submucous, that is, growing chiefly into the uterine cavity. Now, we have not yet arrived at such perfection in therapeutics as to be able to soothe the pain of particular nerves or sets of nerves by the administration of a particular drug; we can only act upon any given set of nerves through the general nervous system, spinal or sympathetic, or by topical application. In the absence of this special knowledge, it seems desirable, and in my experience has been fairly successful, to apply the anodyne as nearly as we can to the seat of pain. Hence the

employment of medicated vaginal pessaries; but, inasmuch as it is no part of the function of the vaginal mucous membrane to digest fats, and as fats without digestion cannot be absorbed, and are apt, moreover, to hinder the absorption of other substances, it is desirable, I think, that we should not use greasy substances of any kind. For this reason I long ago gave up the employment of cocoa butter, and I now invariably use, as the basis of the pessary, gelatine and glycerine in the proportion of one part of the former to four of the latter; into this we can put any ingredient we wish, and I know no better anodynes than atropine, morphine, or conia. Other agents of this class may of course be used; and if it be preferred, they may be given either subcutaneously, or by the mouth, or by the rectum. It has seemed to me that, when given *per vaginum*, they are more effective, and certainly do not produce so much constitutional disturbance as when given in the other ways. For the treatment of what I may call mechanical pain, of course mechanical remedies will also be required. I allude chiefly or entirely to external support, for I cannot imagine that any one would advocate the use of instruments internally in the treatment of these cases.

Next, as regards the hemorrhage, the symptom which, of all others, we shall probably be called upon most frequently to treat. Here there is certainly no one remedy which is applicable to all cases; perhaps I might almost say that there is no remedy which is applicable to any case, for in truth the treatment of this symptom is, as a general rule, most unsatisfactory and disappointing. There is, however, a very general consensus of opinion in favour of ergot, and I certainly know no drug which excels its hemostatic properties in these cases. I believe that it is of most value where the tumour is more interstitial than submucous; it fails, therefore, not unfrequently in the very cases where it is most needed, for it is in the submucous varieties that we get the greatest amount of hemorrhage. The cause of this failure, or rather of this partial success, is apparent when we consider that the vessels are larger and more numerous in the substance of the uterus than in its mucous surface; and further, the special action of the drug is greater in the substance of the uterus than in its mucous lining; in the one case, it can control blood-supply by diminishing the calibre of large vessels; in the other, the tissue to be acted upon, viz., the capillaries in the mucous membrane, lie, as it were, outside the range of its action.

In the great majority of cases, however, hemorrhage, especially when it occurs to any extent, is not merely of mucous origin; still less is it solely interstitial, but it comes from both sources, and hence it is that a combination of ergot with any

purely astringent hemostatic answers better than either singly. Moreover, in consequence of the great losses sustained, and the necessary impoverishment of the blood which remains, anæmia more or less marked is sure to follow: chalybeates, therefore, in some form or other seem plainly indicated. Hence the rule with me, in the great majority of cases, is to administer ergot with the peracetate of iron. I find this combination the most generally successful; but iron alum, or any other astringent preparation of iron, will probably answer as well. I need not particularise other forms of hemostatics, for their name is legion; but I very much question if their value be at all proportionate to their number or variety. In many cases, none of them will suffice; all are equally ineffectual. Then it may be necessary to resort to topical applications, and I know none which is so effective as the small solid stick of anhydrous sulphate of zinc (No. 5 size), first introduced into practice by Dr. Braxton Hicks. I greatly prefer this to the use of fluid injections into the uterus, as I believe it to be fully as useful, and far less dangerous. Through the speculum it may, without any difficulty, be passed quite up into the uterine cavity. The plan recommended by Mr. Baker Brown of freely incising the cervix for the purpose of curing the hemorrhage, I have on several occasions tried, but I cannot say that I have ever seen any good result from it.—*Brit. Med. Jour.*, *Dec.* 2, 1871, *p.* 636.

---

## 96.—MULTILOCULAR OVARIAN CYST NEVER TAPPED: OVARIOTOMY; RECOVERY.

By Dr. JOHN C. GOODING, Cheltenham.

[The patient was sixty years of age, and unmarried. She had always had excellent health. The symptoms of the disease dated back eight years. Just before the operation was performed the abdomen was entirely filled by a fluctuating tumour, extending above the ensiform cartilage; the uterus was mobile, and in its normal condition; the tumour could not be felt by the vagina.]

June 15th, 1871. The patient is in excellent health, and was out even up to yesterday making little purchases in preparation for her seclusion. She is hopeful, but resigned. The bowels acted during the night, and the bladder was emptied before the operation. Beef-tea was given three hours, and brandy-and-water immediately, before the administration of the chloroform, which was kindly undertaken by Mr. C. J. Bennett. The patient, warmly clad, was conveniently placed on a table in a room the temperature of which was 70° F. An incision, three inches long, was carried midway between the

umbilicus and symphysis pubis, through the skin, fasciæ, and rectus, down to the peritoneum, which was then divided on a director. The hand, introduced between the tumour and abdominal walls, found no adhesions, not even in the left iliac region, where I expected there might have been some resulting from the recent inflammatory attack. The cyst was punctured, and withdrawn until it resisted gentle traction. To introduce the hand to determine the detaining cause it was necessary to extend the incision by an inch, as the partially extruded cyst occupied a portion of the wound. I found, high up in the epigastrium another cyst, the contents of which were evacuated through the first, and then the whole growth was easily withdrawn. The pedicle, very short and thin and two inches broad, was transfixed through a translucent part and tied in two portions with stout silk, severed, and replaced in the pelvis. The other ovary was healthy. The insignificant oozing from the divided rectus was sponged away; no fluid having escaped into the abdomen (thanks to the efficient assistance of Messrs. J. Humphreys and C. J. Newton). The wound was closed by five silver sutures traversing the peritoneum at least half an inch from its cut edges, and two or three superficial wire sutures and broad strips of plaster. A large pad of cotton wool, retained by other strips of plaster, filled and supported the concave abdomen and completed the dressing. The patient was replaced in bed; pulse 85, good.

The tumour was made up of two large cysts, which between them held twenty-five pints of thick dark fluid; a third cyst, about the size of an orange, containing glairy white fluid; and numerous small cysts, embedded in and projecting from the interior of the walls of the larger cysts, and crowding round the pedicle. When the cysts were all emptied, the solid portion weighed 2 lb. 10 oz.

Four hours after the operation the patient was comfortable; had felt slight nausea, which was completely allayed by ice; the pulse was 96; skin warm and perspiring. Eight ounces of urine drawn off. One tablespoonful of cold milk was given, and ice only ordered for the next few hours. At midnight—eight hours after the operation—I found that the patient had slept soundly for an hour; her aspect was good; pulse 90; skin warm and moist. Six ounces of urine drawn off. On the following day I found that the patient had taken thirty minims of nepenthe to relieve slight pain occasioned by the vermicular action of the intestines; she had slept well, and was comfortable. A teacupful of milk, and half that quantity of beef-tea, had been taken during the past twenty-four hours. On the third day flatus passed, the patient continuing her favourable progress. On the seventh day all the

sutures were removed, as the wound was found to have healed throughout its entire length; but at the upper angle, from the want of another superficial suture, one lip was more elevated than the other, exposing half an inch of raw surface. A piece of lint soaked in carbolic acid lotion, and long strips of plaster, embracing the hips, were applied; the wool and plaster as before to support the abdomen, which continued undistended. The use of the catheter was continued up to the tenth day, from the patient's inability voluntarily to evacuate the bladder. The bowels were relieved by castor oil on the ninth day. On the twelfth day a tonic mixture was ordered, to stimulate her appetite; on the fourteenth she was allowed to sit up in bed; and on the twenty-first, the wound having for some days past completely healed, she took an airing in a wheel chair. A few days after she went into the country; and on her return recently came to see me, and was looking exceedingly well.

To tie and return the pedicle certainly seems the next best mode of dealing with it, when, owing to its shortness, the clamp cannot be used. The portion of the pedicle on the distal side of the ligature—the stump,—surrounded as it is by *warm* tissues, no doubt retains its vitality long enough for it to become attached by lymph—rapidly effused and organised as we know this to be—to adjacent parts. And in the same way that a completely detached portion of lip, if quickly re-adjusted, *and its warmth be maintained*, will soon become part of the body again, so does the stump become vivified by blood conveyed to it through the newly formed vessels rapidly developed in the effused lymph. The ligature, when tightened, buries itself too, and brings into apposition the peritoneal covering of the pedicle on either side of it, and between these adhesion probably soon takes place. The *material* of the ligature would scarcely seem, theoretically, to affect the result, for by the complete closure of the abdominal wound air is excluded and decomposition prevented; there being then no putrefying fluid for the ligature to absorb, hemp and silk would be on an equality with metal. The results of cases by those who have had many opportunities of treating the pedicle as in this case seem, practically, to favour this view.—*Lancet, Dec.* 2, 1871, *p.* 776.

---

## 97.—A SUCCESSFUL METHOD OF TREATING CERTAIN CASES OF DYSMENORRHŒA AND STERILITY.

By Dr. J. Protheroe Smith, Senior Physician to the Hospital for Women, London.

[The dependence of dysmenorrhœa upon a mechanical constriction of the cervical canal of the uterus was first pointed out by

Dr. Mackintosh, of Edinburgh, in 1844. In 1847, Sir James Simpson described his operation of incising the cervix.]

The instrument used by Sir James was a kind of *lithotome cache*, by which he divided the strictured part, the incision commencing at the union of the cervix with the body of the uterus, and passing more and more into the substance of the cervix as the instrument was withdrawn, and so dividing its lower edge.

In the year following, I had a double hysterotome constructed in Paris, with two blades cutting laterally; and Dr. Greenhalgh subsequently contrived a very ingenious instrument of this description, by which he proposed to gauge accurately the extent of the incisions on both sides. Dr. Black, however, observes that—"In consequence of the gratifying results sometimes produced by incision of the cervix uteri in obstructive dysmenorrhœa and sterility, that operation became an extremely favourite one with Simpson. Certain risks connected with it, however, and, in particular, its liability to be followed by pelvic inflammation, inclined him ultimately to a rigorous selection of cases, and to the enjoining of recumbency for several days after the performance of the operation."

Stimulated by Sir J. Simpson's favourable opinion, for some time I followed this mode of treatment; but the results, as regards the removal of dysmenorrhœa and sterility, were not such as to convince me of the advantage of its general adoption, On the contrary, in several instances, severe metrocellulitis occurred, together with considerable hemorrhage, requiring the plug to control it. In others, the painful menstruation was unrelieved, and the patient remained sterile. My objection, however, to the use of the hysterotome was confirmed by observing that, in several instances, it had aggravated the evil; for not only did the patient continue barren, but the original constriction was increased, and, with this straitened state of the uterine canal, the consequent distress was augmented. The pathological condition I discovered by the use of sea-tangle tents. The first case which yielded this information to me was one in which Sir J. Simpson had performed the operation some years previously. For a time, after recovery from its immediate effects, relief from the distressing symptoms was experienced by the patient; but after a short time the dysmenorrhœal sufferings returned, and, as they gradually increased in severity, she was led to consult me. Finding that the uterine sound could be passed only with extreme difficulty through the os internum uteri, I introduced a small sea-tangle tent, which, on the following day, I had considerable difficulty in withdrawing. It was only after continued traction, kept up steadily for nearly twenty minutes, that this was accomplished. When removed from the grasp of the cervix, its form explained the difficulty.

At an inch and a half from its proximate end, the tent was not altered in form from its size before it was introduced; whilst above and below this narrow waist, it had swollen to its utmost extent; from this compressed part ran a narrow groove on each side, evidently showing that, at the constricted os internum, as well as longitudinally on each side, there existed unyielding tissue, not only at the points of the original stricture, but also at lines corresponding with the wounds made by the hysterotome, which led me to the conclusion implied in these remarks —viz., that the deep grooves and the longitudinal furrows were the impression of hard cicatrices, the effects of the previous operation. Seeing that the result of incising the cervix, if successful in enlarging the canal, must be to destroy the perfectness of its structure to the extent of the tissue divided by the hysterotome, or that, if rendered unsuccessful by the healing of the wound, it might leave the patient worse than before, from the hardened cicatrix which might follow the operation, I soon adopted a course which I have pursued for many years with a success which encourages me to hope that its announcement will be acceptable to obstetric practitioners generally.

In my lectures, at the commencement of this year, on flexions, torsions, and displacements of the uterus, at the Hospital for Women, I observed that, "finding sterility and dysmenorrhœa were often benefited by dilating the cervix uteri by the bougie, after the manner advocated by Dr. Mackintosh, and with a view of improving upon the plan suggested, I got Messrs. Fergusson, instrument-makers to St. Bartholomew's Hospital, to make me in 1841 the instrument I now exhibit, after the model of Heurteloup's lithotrite, by which the extent and direction of the uterine cavity was easily measured, and a constriction in any part of the passage as readily overcome." With this uterine dilator, I conceived it might be practicable to dilate permanently the constricted os internum, and afterwards, when necessary, to give the normal shape to the os tincæ by dividing it latterly at the commissures of the labia uteri *per speculum*. I have now adopted this mode of treatment for more than a quarter of a century; and although, in some instances, I have failed to cure, yet I have succeeded in so many others, that I feel warranted in advocating its adoption in the selected cases in which it is eligible. To diagnose such, it has been my custom to ascertain from the history of each case, as well as by a careful physical examination of the uterus, that it is one simply of stricture of the os internum, and narrowing of the cervical canal and mouth. But should there be any inflammatory condition, whether of the mucous lining or of the deeper tissues of the uterus, or any thickening or induration of these parts, it is essential to success, however tedious the process may be, to remove such morbid

conditions before adopting the *extension forcé*, by which the stricture is to be overcome.

The plan which I then pursue is, first to prepare the patient by a purgative dose, and by abstinence from local excitement, and from alcoholic drinks, or much animal food. When hyperæmia exists, I scarify laterally the labia uteri repeatedly at the commissures of the labia, by which the vascularity of the organ is reduced, and the shape of the os tincæ, when constricted, is improved. After accustoming the uterine canal to bear a metal bougie, which should be repeatedly and daily introduced, and increased in size until that of a No. 10 catheter can be borne without any pain, then the uterine dilator may safely be employed. It will be seen that this instrument consists of two short blades, two inches and a half long, the inner being continuous with the sliding shaft, with which it is nearly at a right angle, having at its proximate end a screw worked by a nut so as to mark precisely, by an index on the handle, the extent of dilatation employed. This should be used at first cautiously about every second day, always ceasing to screw as soon as pain is experienced. This is immediately relieved by a turn or two of the screw the reverse way.

It will be found, in a short time, that the uterus becomes accustomed to the dilatation, when it may be employed to a greater extent; and in the course of a few days or weeks, as the case may be, a forced dilatation to the extent of an inch or an inch and a half may be used with impunity. After this it will only be necessary to use the dilator daily for two or three days, and afterwards at longer intervals, to keep the parts open till they permanently heal in the state of distension effected by the operation.

Should any congestion or inflammation result, scarifying at the commissures of the labia will relieve by free bleeding, whilst, at the same time, the os tincæ is made to assume a more open, and, therefore, a more normal shape. I prefer, generally, to effect this by the repeated use of a small scimiter-shaped knife, as I find that by so doing the risk of inflammation is diminished, and it prevents cohesion of the cut sides of the labia uteri, both which accidents occasionally attend the operation when performed at once by a hysteratome. When preternatural shortness of the uterus, from original malformation, exists, the operation is contraindicated; also when stricture depends on endometritis; when, otherwise, metritis or metro-cellulitis is present; when there are fibroid tumours causing inflammatory adhesions; when dysmenorrhœa is characterised by deciduous membranes; when there are conical hypertrophy and elongation of the cervix; when there is globular enlargement of the anterior labium uteri, embraced by the posterior

lip in the form of a crescentic membrane; and when displacements and dislocations of the organ complicate the case—these and all other organic diseases which may attend this malady should be removed prior to the adoption of forcible extension by the dilator.—*British Medical Journal, Dec.* 16, 1871, *p.* 694.

---

## 98.—ON THE TREATMENT OF SIMPLE MENORRHAGIA AND SIMPLE METRORRHAGIA.

By Dr. J. Matthews Duncan, Edinburgh.

The treatment of simple menorrhagia and of simple metrorrhagia is a very easy matter, so far as it goes. It is divided into the treatment before the loss begins, and the treatment while the loss is going on.

The former treatment, that before the loss begins, is only possible in chronic cases, or cases where there has been at least one previous hemorrhage; for in any other its occurrence has not been and cannot be foreseen. It consists in the application of remedies to any condition which may appear to predispose to, or to aid in maintaining, the complaint. Congestion of pelvic vessels may be diminished by saline laxatives. Congestion of the portal system may be diminished by so-called antibilious medicines. A state of anæmia may be diminished by the use of chalybeate medicines. The general health may be improved by remedies or by hygienic regulations.

The great treatment or chief treatment is that applied during the continuance of the flow. It is of various kinds.

Mechanical treatment is most valuable, and probably as efficacious as any other. It consists, first, in maintaining rest in the horizontal position. By this means the pressure of blood on the vessels of the uterine mucous membrane is diminished, and the bleeding proportionally discouraged. This treatment is favoured by keeping the patient cool. It is applicable in every case. A second kind of mechanical treatment, that by the vaginal plug, is only applicable in severe cases in women who have cohabited, or in the very severest cases where cohabitation has not occurred. This remedy discourages the bleeding by impeding or resisting its escape. The vaginal plug generally resorted to in cases of simple menorrhagia or simple metrorrhagia is of an imperfect kind, yet often efficient: that is, it is not sufficient absolutely to prevent or arrest hemorrhage which proceeds with any great force, as would be the case, for instance, were an artery opened by incision; but when the hemorrhage may be, as it often justly is, likened to an oozing, its action is powerful enough to arrest it. This imperfect plugging is effected by pushing adroitly into the vagina a piece

of sponge as big as the fist when not compressed, or even bigger. For the sponge, bits of lint may be substituted, and inserted one after another. It is common to dip the plug, before using it, in vinegar and water, or in a weak solution of alum. It is, no doubt, possible to explain the action of the plug, by supposing that it excites contraction in the muscular tissue surrounding the uterine vessels, which contraction will restrain the hemorrhage; but the purely mechanical explanation is more in accordance with ordinary medical thought. The plug must be removed in twelve, or, at most, twenty-four hours, and a new one placed, if it is required. More thorough and powerful plugging is very seldom used in cases of the kind under description. To effect it, the vagina is crammed with pieces of lint so as completely to occlude it. Each piece is, when rolled up, about the size of a walnut, and is introduced separately as high as it can be pushed. If the lint is previously dipped in a solution of perchloride of iron, the plug is more efficient, probably because it soon gets hard in the vagina, and more powerfully resists the tendency to hemorrhage. But this dipping in solution of perchloride of iron makes the plug more difficult and painful to remove.

Another kind of treatment resorted to only in extreme cases is probably also mechanical in its action. This consists in painting the bleeding surface—that is, the mucous membrane of the cavity of the body of the uterus—with perchloride of iron. A syringe, containing about a drachm of the pharmacopœial solution or liquor of perchloride of iron, is fitted to a hollow uterine probe after it has been passed up to the fundus uteri; the iron is then gently injected into the uterine cavity; only what is easily thrown in being passed. This hardens and destroys the mucous surface, and so arrests the oozing hemorrhage. The superficial layer of mucous membrane is sometimes subsequently thrown off like the membrane of dysmenorrhœa. I have observed the application and efficiency of this remedy with the naked eye in a case of chronic inversion of the uterus, with excessive menorrhagic loss.

The local application of cold is much esteemed as a remedy by many practitioners, but I have so often seen it fail, and so often seen it apparently increase the loss, that I am very doubtful as to its real value. Cold is, with this view, applied to the lower part of the belly, the vulva, the insides of the thighs, and occasionally even to the interior of the vagina and rectum. It is done by using cloths, wrung out of cold or iced water, and frequently rewetted and reapplied, or by the use of suitable mackintosh or indiarubber bags filled with pounded ice. To the inside of the vagina and rectum cold is applied by injection of iced water, or by the insertion of pieces of ice whose edges

are well rounded. This use of cold is probably derived from the application of the same remedy in post-partum hemorrhage. When used in post-partum hemorrhage, as it is in cases of simple menorrhagia, its value has always appeared to me very doubtful. But in cases of post-partum flooding it has another invaluable application, which is probably the parent of all the comparatively inefficient uses of it which we have described or referred to. Its sudden application, acting diastaltically or through the reflex system of nerves, will often excite an obstinately inert uterus to activity at once when friction and kneading of the uterus have failed; and this single excitement of uterine action may be sufficient. After this result of suddenness of application is gained, the repeated use of it, or its continued use, is comparatively inefficient; and it is this continued use only which is resorted to in cases of menorrhagia or metrorrhagia.

The lapse of hours or days, during which the patient rests, brings the arrestment of most cases of simple menorrhagia or simple metrorrhagia. At the same time the physician administers remedies, and while he thus comforts his patient, himself blindly supposes he is contributing to what he calls her cure. It is the so-called hæmostatic medicines that are universally resorted to in cases of this kind. There is nothing worthy of the name of evidence to show that they act. Like my brethren, I use them, and believe they have some slight influence in diminishing or arresting the discharge. They comprise the mineral acids, especially the sulphuric, ergot of rye, digitalis, gallic and tannic acids, and many others.

Dilute sulphuric acid is the hæmostatic medicine in which I have most confidence. It is often given in the form of the infusion of roses. To find what it can do, it may be necessary to use it freely. From ten to twenty drops properly conveyed may be given from four to eight times daily.

Ergot of rye and digitalis I have used in many ways, and, having no reason to prefer one way to another, must leave the details of their administration to the intelligence of the practitioner or to his caprice.

The chances of therapeutical benefit to the patient may be increased by combining the ergot of rye with the dilute sulphuric acid, or the digitalis with the same.

Gallic acid is probably more extensively used during the loss in these diseases than any other remedy. It is given in doses of from ten to twenty grains three or four times daily or oftener. It has the advantage over the mineral acids of not setting the teeth on edge; but it is, I believe, far less useful than they are. The same remarks apply to tannic acid.—*Edinburgh Medical Journal, March* 1872, *p.* 774.

## 99.—THE VALUE OF ARSENIC IN MENORRHAGIA AND LEUCORRHŒA.

By Dr. J. H. Aveling, Fellow of the Obstetrical Society of London, &c.

[In properly selected cases of menorrhagia and leucorrhœa arsenic is a remedy of great value. Its use was suggested to Dr. Aveling many years ago by a remark in a work on midwifery, stating that a teaspoonful of Fowler's solution of arsenic is of service in hemorrhage after delivery.]

Numerous as were the disorders treated by arsenic in the beginning of the present century, it was not until 1838 that its employment was, as far as I am able to discover, first suggested in cases of menorrhagia. In March of that year, a paper was read before the Royal Medical and Chirurgical Society of London, on the Use of Arsenic in some Affections of the Uterus, by Dr. Henry Hunt of Dartmouth, now of London. In 1834, he administered to a woman suffering from cancer of the uterus four drops of the liquor arsenicalis three times a day, gradually increasing the dose to ten drops; and found that, as the poisonous effects of the mineral presented themselves, in the same proportion the pain in the womb subsided. This fact, coupled with the recollection that inflammation of the genitals sometimes follows administration of arsenic as a poison, induced him to hope that it might be useful in some disorders of those parts. He was further strengthened in this opinion by the experiences of his father and of Sir Charles Locock. The latter told him that he had cured a lady of menorrhagia by arsenic, having recommended it to her for a disorder of the nose, being ignorant at the time that she was subject to the former disorder, she having neglected to mention it, considering it quite irremediable, as it had baffled the skill of every physician she had hitherto consulted. Dr. Hunt's father had met with a similar case in a girl of seventeen, who had consulted him for a leprous eruption on the knees and elbows, for which he had prescribed three drops of the liquor arsenicalis three times a day. At the end of three months, her mother called on him to thank him for not only curing her daughter of the eruption, but for making her regular, she having menstruated too frequently and profusely. Dr. Hunt, after relating several cases in which arsenic had proved successful in his own practice, says that, from the immediate and progressive improvement which succeeded the administration of the arsenic, the cessation of the menorrhagia may be fairly attributed to the action of that medicine. He explains the benefit derived from it as being due to its stimulating action upon the mucous membrane; and states, consequently, that it can be given with

greatest advantage in those disordered conditions of the uterus which have been induced and kept up by debilitating causes.

It is remarkable that, notwithstanding the clear manner in which the claims of arsenic have been urged by Dr. Hunt, it is still little used by gynæcologists. One may look through book after book, and no mention of its employment will be found. In two of the most complete recent systematic works on the diseases of women, it is referred to in the following brief sentences. "The arsenical preparations," remarks Dr. Courty, "appear to me to be followed, after long usage, by an advantageous result, when there seems to be a relation between the uterine disorder and a herpetic diathesis." Dr. Fleetwood Churchill simply says, "Arsenic has been tried with success in menorrhagia and cancer uteri by Mr. Hunt." In 1858, Dr. James Begbie quoted, in the Edinburgh Medical Journal, the experience of Dr. Hunt, and stated that both Sir James Simpson and himself had used arsenic with success in uterine complaints. Professor Hardy recommends arsenical medication in leucorrhœa of the neck of the womb; and Dr. Barnes tells me that he has used the same remedy in cases of menorrhagia. I am happy to be able to add the name of another author who has not neglected to mention the virtues of arsenic. Dr. Tilt, in his Handbook of Uterine Therapeutics, says: "When we consider the structural analogies that exist between the skin and the mucous membrane, it is surprising that arsenic has not been more frequently tried. I have given it with good results in cases of chronic uterine subacute inflammation with marked tendency to relapses." He does not agree with Dr. Courty in inferring that a disease of the womb must be herpetic because it yields to arsenic. In the Proceedings of the Gynæcological Society of Boston, U.S., Dr. Wells relates a case of spasmodic menorrhagia in which the arsenic treatment succeeded admirably.

The arsenical waters of Bagnères-de-Bigorre have been used for some time in the treatment of these diseases. Dr. Macpherson informs me that the arsenical water of Whitbeck, on the Furness and Whitehaven Railway, has been highly recommended as a therapeutical agent by Dr. Robinson of Newcastle; but I am not aware whether its effects have been tried in the disorders we are now considering.

I will not detain you longer with further historical remarks, but will at once proceed to give you the results of my own experience.

The preparations I have usually employed are two—the liquor arsenicalis; and the arsenious acid in granules, each containing one *milligramme*. This latter is an elegant form of administering the remedy; for, as it has to be taken at meal-

times, the granules can be placed on the tablecloth, wrapped in a morsel of bread, and swallowed unobserved. Considerable difference of opinion exists as to the best mode of giving arsenic, some employing large doses in quick succession, and others small, extending over a long period. Aran says: "The rapid mode of administration is better than the long continuance of small doses, because the economy habituates itself to the latter, and the therapeutical effects may be lost; and the proceeding has the additional disadvantage of leading more easily to the saturation of the economy, and consequently to intolerance." On the other hand, Sir James Simpson says: "Most reliance ought to be placed on small and very long continued doses of arsenic; and it is infinitely better and safer to trust to the curative effect of the long continuance of such small doses of this remedy, than to attempt to arrive at the same result by throwing in larger doses for a shorter time." Dr. Hunt says: "Large doses taken for a short time produce much distress, without the desired effect on the uterus." The plan of small and long continued doses is the one which I have always used; and the result has been so satisfactory, that I have never thought of adopting the more rapid method. The doses with which I commence are from two to six drops of the solution, and from one to three of the granules, three times a day, at meal-times. These are small doses, when we remember that a Styrian arsenic-eater has been known to take as much as five and a half grains of arsenic at once; but they are strong enough to commence with, and may be increased from time to time as the necessities of the case suggest, and the patient's capability of bearing the remedy permits. It is advisable to suspend its administration occasionally for a short time. This, indeed, may sometimes be necessary, should diarrhœa, nausea, or pains in the stomach, supervene. It is also better not to discontinue the doses abruptly. They should be gradually diminished in quantity, and taken less frequently.

The first effect of arsenic is to improve the digestive powers. The appetite returns; and often in two or three weeks the patient has improved in appearance and increased in weight. Besides this improvement of nutrition, there is soon evident increased tone of the nervous system. Respiration and secretion are better carried on; and M. Lolliot has established that ten *milligrammes* of arsenious acid, taken each day, produce lowness of temperature and diminution of urea. The Styrian arsenic-eaters are generally strong and healthy persons, courageous, and of strong sexual disposition. If requested to explain why they take arsenic, they say it is to make them strong and healthy, and to improve their wind in ascending mountains. M. Isnard, in his work on Arsenic in Diseases of

the Nervous System, shows that it replaces altogether sedatives, antispasmodics, and tonics, calming pain, spasms, and convulsions, and stimulating depression of the nervous force. For my own part, I think the most valuable therapeutic effect of arsenic is its decongestive action upon mucous membranes.

But, before endeavouring to explain the mode in which arsenic effects a cure in cases of menorrhagia and leucorrhœa, it would be well to examine the primary pathological condition of the uterus which causes them. This condition is, in a great majority of cases, one of hyperemy, which has been defined by Andral, the inventor of the word, as excess of blood in the capillaries. Hyperæmia of the uterus may have a physiological or a morbid origin—physiological, when caused by sexual excitement, menstruation, or pregnancy; morbid, when it is the result of pre- or post-inflammatory action, of traumatic, chemical, or morbid irritation, of an atonic, obstructive, or hypostatic cause, or of heat, cold, &c. Of course I do not wish to deny that menorrhagia and leucorrhœa may be produced by polypi, muscular fibroids, cancer, and many other pathological conditions; but I would at the present time draw attention more particularly to those forms of menorrhagia and leucorrhœa which have a hyperæmic origin, because it is in these that arsenic will be found most efficacious. In short, it is the morbid condition, of which menorrhagia and leucorrhœa are but the secondary phenomena, which I propose to treat with arsenic; for, if we can cure the former, the two latter must necessarily disappear. Yet I would not have it understood that these consequent symptoms are to receive no attention. Both must be checked when excessive. All gynæcologists, however, know how imprudent and injurious it is to stop abruptly discharges which are often nothing more than Nature's method of relieving the hyperæmic condition of the parts from which they emanate. Hyperæmia of the passive or atonic character is that which is most benefited by the use of arsenic. The uterus, when in this condition, is larger and softer than in its normal state. It is usually tender to the touch, but not always so. To the eye it appears of a deeper red than is natural. After death, the capillaries are found dilated, and the tissues tinged with red. Unlike the colour produced by inflammation, however, this redness can be removed by careful washing. A patient coming to you with her uterus in the state just described, will, in addition to a host of other subjective and objective symptoms, most probably complain of the too frequent recurrence of the catamenial period, of the excessive discharge at that time, and, in the intercatamenial period, of persistent and distressing leucorrhœal flow. Now, in such a case as this, I should commence by administering two drops

of the liquor arsenicalis, or one granule of arsenious acid, three times a day, at meal-times. This dose I should continue for a fortnight. If, at the end of that time, no conjunctival irritation had displayed itself, I should increase the dose to four drops of the solution or two of the granules; and then again, after another interval, to six, eight, ten, or even more drops, or granules in proportion, watching the patient, and being guided by her tolerence of the remedy.

Besides the general effects of arsenic already alluded to, the first result of this treatment will be the lengthening of the intercatamenial period; and it is remarkable how gradually this is sometimes extended, one or two days being only gained at a time. By persisting in the remedy, however, the interval will become greater until it arrives at its normal duration. Occasionally the progress is more rapid, and the proper interval is at once attained. Besides the improvement in this respect, the amount of the discharges will gradually decrease, and in like manner all the other hyperæmic symptoms disappear. I have never found it necessary to administer large doses, and cannot remember ever having produced any of the premonitory symptoms of arsenical poisoning beyond that of conjunctival tenderness. I have been obliged, however, to continue the remedy for several months, and have had to recur to its use more than once when the hyperæmic symptoms have reappeared. In some cases, an excessive leucorrhœal discharge has the effect of supplanting the catamenial. In these the cure of the former has the result of removing the amenorrhœa. The late Dr. Wright says: "Arsenic has succeeded in my own practice when a long succession of other remedies has previously failed to induce or re-establish menstruation." If I am asked to explain what is the therapeutical action of arsenic in these cases, I frankly own my ignorance, and admit that, like many other medicines, I use it empirically, and patiently await the explanation which science may some day be able to give. Theories are not, however, wanting. Dr. Hunt, judging from the fact that inflammation of the genital organs has been produced in some cases of arsenical poisoning, infers that the action of arsenic in curing utering disorders "may be explained by its acting on the mucous membrane of the uterus as a stimulant." Dr. James Begbie, believing the uterus to be affected in its functions and structure through rheumatic and other morbid conditions of the blood, says that "the efficacy of arsenic resides in its powerful alterative effects upon the blood." Another theory, which, without denying the possible truth of the two foregoing, I feel most inclined to adopt, is, that the arsenic, circulating with the blood, acts upon the vaso-motor nerves of the capillaries as a stimulant and tonic, causing them to contract

and expel the superabundant blood. But, whatever theory may be correct, the practical results of the administration of arsenic in menorrhagia and leucorrhœa are most satisfactory; and I shall be glad if these few remarks have the effect of once more drawing the attention of the profession to a safe and potent remedy, by the means of which two of the most common and distressing complaints from which women suffer may be brought under subjection and finally cured.—*British Medical Journal, Jan.* 6, 1872, *p.* 10.

---

## 100.—ON MENSTRUAL COAGULA.

By Dr. John Haddon, M.A., Eccles.

Abortion is a very convenient name, and under it are classed many cases which have no claim to such a designation.

It is not so generally known as it ought to be, that blood may be retained in the uterus for one, two, or more months, without any untoward symptoms, until hemorrhage begins, and the clot is expelled with pain and flooding.

Such cases as these may have, no doubt, the history, as well as some of the signs and symptoms, of abortion; and even the product may be mistaken for an aborted ovum—more especially if any of the decidual uterine membrane, which is seen in membranous dysmenorrhœa, be present.

I had not long been in general practice before meeting with cases which I supposed to be abortions, where, nevertheless, I could find no trace of any part of the ovum. There is no great harm in telling a woman in married life that she has aborted, where the symptoms of abortion were present, even though no trace of the ovum can be discovered. When it happens, however, as it did to me, that you meet with such a case in an unmarried young lady, you must pause and consider the case very minutely before even hinting at the possible occurrence of conception. I shall relate the case which led me to suspect that abortion may be in many cases simulated by an affection having its origin in some peculiar state of the menstrual function.

A. H., aged 28 years, and unmarried, states that, when sixteen years old, she had a fever, during which menstruation began. It continued normal till about the age of twenty, when, after sitting on damp grass, she had an affection of the uterus, which confined her to bed for some weeks. Since that time menstruation, though regular, has been attended with considerable pain, and followed by more or less leucorrhœa. She has also been subject to hysterical attacks in the form of convulsions. From April 1870, menstruation was regular every month, lasting

about three days, and followed by copious leucorrhœa, which continued during the interval. She also complained of pain in the back and in the left side, was unable to walk much, and very subject to hysterical convulsions. She improved under general and simple local treatment. In June she had no hysteria, very little leucorrhœa, no pain, and was able to walk long distances.

On 12th September, 1870, I was called in the night to see her. I found that, since June, she had been quite regular, with very little leucorrhœa, and had felt stronger than usual. On 1st September, which was her monthly period, menstruation did not occur. She did not feel ill in any way until 12th September. On that day she had pain in the abdomen, and was altogether out of sorts. During the night she became hysterical, and lay in a half-unconscious state. Her pulse was normal. There was no uterine discharge; but she complained of pain in the left inguinal region, which was increased on pressure. Over the tender part there was some fulness, and slight dulness on percussion. Poultices were applied, and she was confined to bed. The pain gradually subsided, but she was advised to stay in bed till the next period had passed. On 30th September she began to menstruate. On 1st October it became very profuse, I found her flooding, and was shown several clots the size of walnuts, which had been discharged. The face was pale and the lips blanched: the pulse was quick and weak. On vaginal examination, the uterus was found low down in the vagina, and quite movable; the cervical walls were thickened, the body larger than natural, and the os just open enough to admit the tip of the forefinger. Ergot was administered, and other means used to stay the flooding, which soon abated. The sanguineous discharge continued. She had occasional sharp pains in the back, and some difficulty in micturition. On the twelfth day from the beginning of the discharge, when it had almost ceased, a mass was passed which in shape resembled a cast of the interior of an enlarged uterus. On vaginal examination at this time, the uterus was higher up and larger than before—as large, indeed, as it would be in a two months' pregnancy. The os was open enough to admit the forefinger. After the appearance of the mass, to be described immediately, the discharge entirely ceased. She menstruated again in the end of October, and has been well since.

*Description of the mass.*—It was slightly pear-shaped, and as large as a small hen's egg. Its consistence was firm, and from its narrower end hung long shreds of fibrin. On section through the middle, a cyst was found in the broader part. This cyst was an inch in length, half an inch in breadth, contained clear serum, and was lined by a smooth shining membrane. Around

the cyst the substance was of a dark red, almost a black, colour; while the outer part was of a firmer consistence, and paler in colour. No organized structure could be detected by the microscope.

Such is an account of the facts of the case, Had it occurred in a married person I should have thought it was an abortion, but as there was not the least suspicion of pregnancy, I was induced to give the case more consideration. There could be no doubt as to the mass expelled being an old clot of blood. What its age may have been is a matter of opinion; and some may doubt whether it was uterine. I am inclined to believe that it was formed in the uterus on 12th September, because when menstruation began the uterus was larger than natural, and continued to enlarge until the mass was expelled.—*Edinburgh Medical Journal, Jan.* 1872, *p.* 611.

---

## 101.—ON THE SIGNIFICANCE OF UTERINE AND VAGINAL DISCHARGES.

By Dr. Robert Barnes, Obstetric Physician to St. Thomas's Hospital, &c.

[Air may get into the vagina, if not into the uterus, in the non-pregnant state. Under peculiar circumstances air enters in large quantities, to be expelled with noise. In one case related by Dr. Hartley there was found to be retroversion of the uterus, and this displacement being remedied, and the patient's health improved, a cure ensued.]

If you observe the vagina when the duck-bill speculum is applied, you will see movements of rise and fall under the influence of the rise and fall of the diaphragm. Dr. Adolph Rasch has investigated the phenomena with great care. He says, if a multipara whose genitals are normal be placed on her back, with the thighs flexed and abducted, and the vaginal orifice closed, movements caused by respiration are seen, but no air enters. In the lateral position the same thing is observed even if the vagina is lax, and even when the perineum is ruptured. When the patient is placed in the prone position, or on all fours, if the vulva be open, air will enter, because the intestines falling downwards by gravity causes a vacuum. Under this condition violent exertion may expel air, giving rise to vaginal flatus. If the abdomen be supported by the hands or by a bandage, no air enters.

There are several interesting applications of this knowledge. It teaches that the best position after labour, if not during labour also, is the dorsal; that the same position is also best in the case of pelvic abscess or hæmatocele discharging into the

vagina; and that we must carefully consider this respiratory rise and fall of the vagina when selecting pessaries. It is by turning to account this action that we derive the greatest advantage from the spoon or Sims's speculum. The blade drawing the perineum well back, whilst the semi-prone position of the patient favours the falling forwards of the abdominal viscera, air fills the vagina, counteracts the effect of inspiration, and thus enables us to get a good view of the os uteri. The same position also greatly aids our efforts at reducing inversion of the uterus, and in replacing a prolapsed umbilical cord. In most operations, however, upon the uterus and vagina, where it is of importance to bring the uterus as low down near the vulva as possible, the dorsal position, by bringing the force of gravity to counteract the respiratory rise of the uterus, which can further be greatly aided by direct pressure by an assistant's hand above the symphysis pubis, is the best.

If we were to follow out in detail all the indications supplied by the examination of the discharges, we should be led through almost the entire range of uterine and vaginal pathology. This, in fact, is simply the converse proposition to one already stated—namely, that almost every uterine or vaginal disease is attended by discharge. But to adopt the discharges as our point of departure, from which to proceed to study and classify the diseases of which they are consequences, would be a most intricate course, and not the most orderly or profitable. I will therefore not pursue this method further than to give one instance of how we may apply it to clinical analysis. We will at our next meeting take hemorrhage, and sketch what its purport is.

*The Watery Discharges.*—When these occur, you must first of all determine the presence or absence of pregnancy. It is no uncommon thing that discharges of water, more or less profuse, take place in pregnant women. This is the hydrorrhœa gravidarum. Gushes of water, quite clear, may occur at almost any time during pregnancy; but they are more frequent in the latter months, and especially in the last month. Happening at this time, they are commonly taken as an indication of commencing labour, and many are the false alarms which patient and doctor have to suffer from this cause. "The waters have broke," says the nurse. You go, as in duty bound, and find probably the os uteri closed, nothing resembling active labour-pains. What are you to do? If you wait for labour, you may wait for a week, or two or three weeks. If, on examination by ballottement, you find the child still floats in the uterus, the os uteri not open, and no active pains, you may go home and wait in peace for another summons.

What is the source and nature of this hydrorrhœa gravi-

darum? Several theories have been expounded. The character of the fluid differs in some respects from that of liquor amnii. It is odourless, and resembles blood-serum or the serous fluid effused in the peritoneal sac. Ruysch and Rœderer thought it came from ruptures of lymphatic vessels, or of hydatids of the uterus; Böhmer thought it escaped from a second abortive ovum; Delamotte and Cruveilhier that it came from a cyst near the ovum; Deleurye, Puzos, Naegele, and Dubois that it came from the inner surface of the uterus, being secreted externally to the ovum. Dubois says it is the result of loosening of the membranes from the uterus when the vessels pour out serum. Hegar says the source is the uterine glands of the decidua. Thus, he describes the glands of the mucous membrane as being found in the decidua at the sixth month of gestation, and argues that their sudden disappearance in the subsequent months is improbable. In a case of hydrorrhœa he found in the decidua vera, at the beginning of the eighth month, an enormously developed glandular body. At the bottom of this morbid growth was a general hypertrophic condition of the decidua and its glands. These gave out the excessive secretions. In a case related by Dr. Graef, repeated discharges took place, and the fœtus was expelled at the end of six months. The membranes were very delicate, and openings were found in them. In this case, it is probable that the fluid was true liquor amnii. In another case the patient suffered, during the last three months, from repeated watery discharges; the uterus rising and falling with the gathering and escape of the fluid. The membranes were found without rent. Graef regarded this as a case of catarrhal hydrorrhœa.

I believe there are various sources. In some cases the fluid is liquor amnii. This may come either from rupture of the membranes; from rapid transudation under pressure from rapid formation and accumulation of liquor amnii in the amnion; or from the bursting of a cyst formed between the amnion and chorion, or between two layers of chorion, the proper amniotic sac remaining intact. In the majority of cases, however, the fluid is not amniotic; for when once the amniotic sac has fairly ruptured, labour is not far off. It is, then, the result of a rapid secretion from the uterine glands or from the cervical cavity. In the early months, whilst there is still a free space between the decidua vera and the decidua reflexa, there is a large area of developed glandular surface.

I have observed a *puerperal form of hydrorrhœa*. Thus watery discharges may continue for a month or longer beyond the proper lochial flow. Generally in these cases the water is dirty, discoloured, occasionally stained with blood, and offensive. The most common cause I have found to be the retention

of a portion of placenta or of clots in the uterus; but a polypus may produce like results. The watery discharges alternate, but not always, with discharges of blood. The fluid may, under certain conditions, collect in considerable quantity in the uterus, so that the organ becomes greatly distended before the collection is expelled in a gush.

Sometimes watery fluid is mingled with air, constituting *physo-hydrometra*. This is also a puerperal or post puerperal condition, and is commonly the result of retention of some portion of placenta or membranes, and the admission of air into the uterine cavity. If an examination is made when the uterus is relaxed after labour, especially if the hand be introduced into the uterus, the vaginal walls are separated from their usual contact, and a channel is formed along which air easily enters. Merely turning on the side, or a little more prone, will often, by favouring the fall of the uterus forwards, produce a vacuum into which air will rush. This is one reason amongst others why I am unable to approve of the abolition of the old-fashioned binder, which some people would condemn, for no better reason that I can see than because it is old-fashioned. After labour, especially in pluriparæ, the abdominal walls are so relaxed that they can give no support to the uterus. The binder does temporary duty for the inert abdominal walls. The history of physo-hydrometra is, I believe, this: a portion of placenta, membranes, or clots, remain in the cavity of the uterus after labour; some air gets in as I have described; decomposition ensues, and the gases of putrefaction are added to the air from without, whilst the os uteri is occluded by the placenta or blood-mass falling over it. When this occurs, there is invariably hectic or irritative fever; peritonitis and septicæmia commonly attend; great abdominal pain; the enlarged, distended uterus can be mapped out rising as high as, or higher than the umbilicus; and resonance is made out on percussion.

One condition, the result of impregnation, often leads to copious and repeated discharges of watery fluid: the *hydatidiform degeneration of the chorion*. In this case the ordinary signs of pregnancy may not be present, and even the patient herself may not think she is pregnant. There is, however, always evidence of enlargement of the uterus, and generally great pelvic distress. The water escapes in gushes at uncertain times; it is often tinged with blood, resembling red currant water; it has not the offensive odour belonging to the watery discharges of cancer; sometimes, but not often until late in the progress of the case, cysts will be found swimming in the water; it is generally expelled with painful uterine contractions. In a case we recently had in Adelaide ward, the nature of the disease was not at first suspected. There was some abdominal

enlargement, retention of urine requiring the catheter, and most distressing pelvic pain with irritative fever. The os uteri was found high up above the symphysis pubis, whilst behind it the pelvic cavity was filled with a large, rounded, firm mass, taken to be either the retroverted gravid womb or a fibroid tumour. One day a large quantity of water, blood, and a mass of chorion-cysts were expelled. We had, in fact, the condition of retroverted gravid womb complicated with hydatidiform or cystic degeneration of the chorion.

Apart from pregnancy, watery discharges are often of grave significance. During and after the climacteric period, the most frequent cause is some form of malignant disease, especially the so-called cauliflower excrescence of the uterus. In this case other symptoms will probably point to the seat and nature of the disease. The fluid discharged is seldom clear; it is generally turbid, dirty, often tinged with blood, resembling water in which flesh has macerated; it contains shreds or flocculi of solid matter, the proceeds of superficial erosion or necrosis of the surface of the diseased growth, and is almost always of a peculiar offensive odour. It often alternates with hemorrhage. Local exploration will place the nature of the case beyond doubt. Another form of malignant disease giving rise to watery discharges is the "oozing excrescence of the labia."

But we must remember that similar discharges may take place from polypus or inversion of the uterus. Hence we have another example of the wisdom of not pronouncing a diagnosis until we have made an internal examination. Water may escape in large quantity from the rupture or perforation of an ovarian cyst into the vagina. In such a case, the rapid concurrent diminution of the abdominal tumour will lead to the right conclusion.—*Lancet, Feb.* 10, 1872, *p.* 177.

---

## 102.—ON HYSTERIA AND ITS INTERPRETERS.

By Dr. Edward John Tilt.

I think it requires two factors for its production—1. A predisposing nervous state; 2. The stimulus of some determining cause.

Of the predisposing cause, we may safely say that it must depend on that modification of the nervous system which makes the nervous system of woman more prone to emotion than her mate; otherwise how is it that the disease is in the main feminine, and only met with in men whose nervous systems are built on the feminine type? We moreover know that, although a disease of every climate and social condition, hysteria is most frequent in women of the upper classes of the

civilised races, in whom emotionalism is intensified, at the expense of reason and self-control, by injudicious training in childhood, and the subsequent pampering that ill fits them for the trials of life. We can go no further than say that this undue action of the brain is the predisposing cause of hysteria. It may be that, in severe cases, this predisposition may be so strong as to be of itself sufficient to bring on the disease. At all events, we know that there are various degrees of intensity in this predisposition, and that the slightest determining cause will make some women hysterical. In a family with which I am intimate there are ten healthy children, whose parents are not in the least nervous; but a paternal uncle is insane; two maternal uncles died of delirium tremens; one brother has been epileptic from childhood; and a sister died of meningitis. Out of these ten children, two little girls—one seven, the other eight years old—burst into tears if they are looked at, if they are not placed as they like at table, and are not helped in their right turn. They pass rapidly from laughter to tears, which will flow for hours and very abundantly. They have sometimes globus hystericus. These symptoms have been repeatedly quelled by preparations of iron; but they occasionally return, and must be taken as evidences of the hysterical state, very likely to be followed by the worst manifestations of the disease on slight provocation.

With regard to the determining causes of hysteria, I must first mention those that intensify all nervous affections—debilitating influences like loss of blood, diseases, physical shocks, mental and emotional shocks, prolonged worry, and want of sleep. Neither should I omit the contagion of one hysterical nervous system on another predisposed to become so.

Coming to the most important causes of hysteria—those originating in the viscera—I will first remark that, as with our mental acts, so with our emotions, they are conceived in the brain; and that old physiology and the poetry of all times have erred in placing the actual origin of our passions in our abdominal organs. Still universal consent shows how strongly they are acted on by emotion—that, in fact, in the viscera are the reflex centres of emotion that stimulate the nervous system to emotional acts.

If I have, therefore, been correct in ascribing hysteria to undue action of the brain as an organ of emotion, a potent cause of hysteria must be found in undue action of one or other of our viscera. It is, no doubt, wonderful that bodies shared by us with the lower animals should not only support the bodily structure, but, by their healthy action on the brain, give lucidity to the mind and warmth to the feelings, making genius more admirable and charity more godlike. This sounds

like poetry, but becomes plain matter of fact when we remember how often anger has caused jaundice, and how frequently a host of distressing mental and emotional sensations are due to that state of liver and stomach derangement that we call biliousness, and which doubtless acts by deranging the functions of the neighbouring great ganglia. I have likewise seen repeated attacks of hysteria brought on by biliousness, and their recurrence prevented by such measures as are best calculated to prevent biliary derangement. Such cases are, however, very rare, when compared with those in which the determining cause of hysteria is an ovarian or uterine ailment. The statistics of Landouzy, Brierre de Boismont, and Dubois d'Amiens, as well as the recent assertions of Dr. Crichton Browne, show this to be the case; and those who deny it must bring forward similar masses of equally well digested facts.

What, then, are the diseases of the sexual system that cause hysteria? Not those in which the structure of the ovary and womb are almost destroyed—acutely, as in abscess of the ovary, slowly, as in ovarian tumours and uterine cancer—but, as a rule, the mildest forms of anæmic ovarian uterine disease; showing that it is not the intensity of the disease that causes hysteria, but the fact of its coincidence with a nervous system prone to become hysterical. Thus hysteria is most frequently caused by those limited ovarian lesions that I have described as sub-acute ovaritis, lesions depending on morbid ovulation, and that frequently pass unrecognised under the disguise of diseases of menstruation. Of uterine affections, it is chiefly the milder sort—that are mucous membrane deep—which cause hysteria; and sometimes, by applying nitrate of silver to an ulcerated cervix, we most unwittingly bring on an attack of hysteria, in patients who presented no signs of its being likely to come on, and thus experimentally prove that the two complaints may stand in relation as cause and effect. On one occasion, I thus brought on an attack in a lady, who had never before had one.

How is the brain, laden with emotion, to be brought into contact with the viscera, the reflex centres of our emotions? The late Dr. Todd thought that hysterical delirium and other hysterical phenomena might be explained by toxæmia resulting from retained menstrual blood; but hysterical phenomena frequently arise before there is any menstrual blood to be retained; and Dr. Handfield Jones agrees with me, that with hysteria, as with other neuroses, there is no blood-poisoning. The distance between the brain and the viscera, between mind and appetite, is bridged by the ganglionic nervous system, which unites the viscera by a federal bond of union, and places this federation in intimate connection with the cerebro-spinal system. When

the ganglionic nerves transmit healthy impressions to the brain, they pass unnoticed; but a hysterical fit shows how differently nerves and ganglia act when visceral action is more or less diseased.

In many hysterical fits, after a period of incubation, in which the system seems to become more and more charged with excitement, the attack begins by pain in the womb and ovaries. Soon the hysterical aura passes to the epigastric ganglia, and, concentrating there, gives rise to the suffocation and distress characteristic of the disease. Ascending still higher, the hysterical aura reaches the cervical ganglia, producing the sense of strangulation; it then attacks the brain, deranging its functions in ways too numerous to be mentioned, and, at the same time, deranging more or less the functions of the spinal cord, according to the degree of tension of the hysterical aura. For a time pain will thus concentrate—sometimes in the visceral ganglia—and the patient collapses into prostration when the system has been sufficiently relieved by convulsions and by critical discharges. It has been possible, in cases published by Romberg and Schulzenberger, to produce the succession of phenomena just described by simply pressing on the ovaries; and I have repeatedly brought on unconsciousness in a nervous patient of mine by pressing the left ovary.—*British Medical Journal, Dec.* 16, 1871, *p.* 691.

---

## 103.—CHLOROFORM AND BICHLORIDE OF METHYLENE IN UTERINE OPERATIONS.

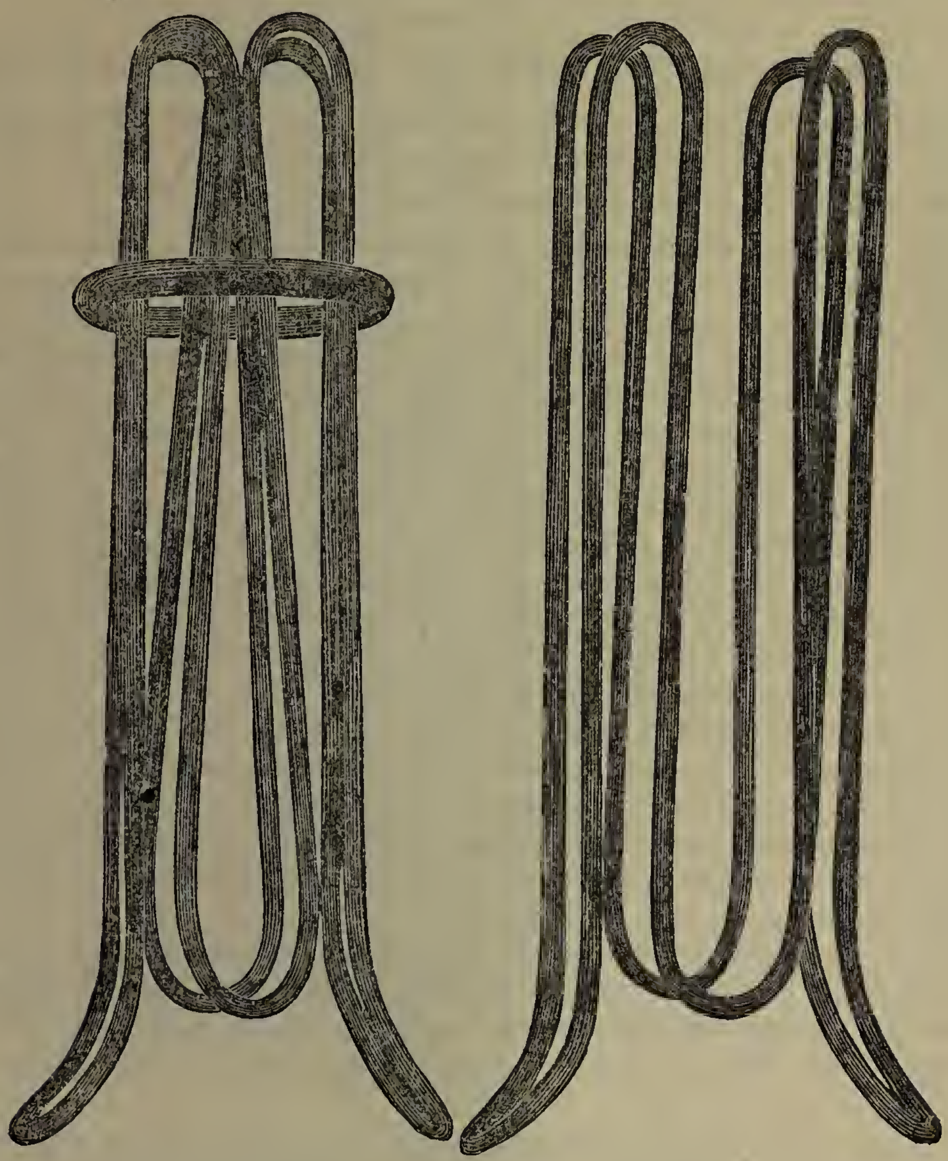
At a meeting of the Royal Medical and Chirurgical Society, Mr. SPENCER WELLS said he had long felt objections to chloroform, especially as regards the vomiting it produced. This was particularly objectionable in many of the operations he had to undertake, especially in the abdomen and vagina. He had tried ether on the advice of Dr. Keith of Edinburgh, but it was so troublesome that he was glad to take to bichloride of methylene. This he thought the best anæsthetic; sickness after it was rare. Some said it was only a mixture of chloroform and hydrochloric ether; that might be so, but as long as it suited his purpose he did not care what it was chemically. It was best given by Jünker's apparatus.—*Lancet, Nov.* 4, 1871, *p.* 638.

---

## 104.—BLACKBEE'S RESILIENT SKELETON SPECULUM.

The engravings here shown present the open and closed views of an entirely new form of speculum, of a most simple and novel construction, and which may be said to possess

great advantages without mechanical complexity. Its chief features are as follows:—1. From its blades—outline or skeleton shape—being composed of hard German silver wire strongly plated with pure silver, it offers the most extensive view possible of the entire circumference of the vagina and cervix



uteri, permitting the easy application of necessary remedies to their whole surfaces. 2. Its resiliency is a great feature in its favour, rendering it easy of introduction and safe in withdrawal, readily adapted, self-adjusting and self-retentive, thus allowing the free use of both hands of the operator. 3. Being free at both extremities, this speculum, by slight manipulation, may be contracted or expanded to suit special cases and requirements. 4. As to durability (having no screws or levers), it may be deemed practically indestructible, as nothing but

absolute violence could possibly injure it. 5. It is extremely light for a metal instrument, and the cheapest speculum yet offered to the medical profession. The ring, useful at first, may, after a little acquaintance, be entirely dispensed with.

This speculum may be obtained of Messrs. Maw, Son, and Thompson, Aldersgate-street.—*Lancet, Dec.* 9, 1871, *p.* 820.

---

## 105.—CONDURANGO—A REPUTED REMEDY FOR CANCER.

[Some wonderful cures of cancer by this drug have caused quite an excitement on the subject in America, and in New York especially it has commanded an enormous price.]

Dr. Bliss, of Washington, is stated to have accomplished by its means several notable cures upon prominent personages, and considers it "as reliable a specific in cancer, scrofula, and other blood diseases, as cinchona and its alkaloids have proved to be in zymotic diseases." The resident practitioners of Ecuador are, however, less confident of its efficacy as a cure for cancer; but believe it to be very useful in cases of rheumatism and secondary syphilitic disorders. Dr. Jaramillo, of Guayaquil, has been eminently successful in curing syphilis, as well as intestinal, urethral, and uterine ulcers; the application being in some cases a decoction of the wood without the bark, but more often of both. Reduced to a powder, he says, an ounce will kill a good-sized dog. The milk of the plant he applied to ulcerated surfaces, and found that it promoted cicatrization. The name *Condurango* or *Cundurango* is said to signify in the Quicha language "Vine of the Condor," from the tradition of the country that, when the condor is bitten by a poisonous serpent, it swallows some of the leaves of the plant and experiences no harm.

[It is perhaps almost needless to add that we do not in any way pledge ourselves to the medicinal efficacy of Condurango; and that as regards its asserted action in cancer we are wholly incredulous. Ed. Pract.]—*Practitioner, April* 1872, *p.* 221.

# INDEX TO VOL. LXV.

# GENERAL INDEX

TO

BRAITHWAITE'S

# RETROSPECT OF MEDICINE,

1869—1871.

VOLS. LIX. TO LXIV. INCLUSIVE.

PRINTED FOR D. I. ROEBUCK,

AT THE OFFICE OF THE YORKSHIRE POST, ALBION STREET, LEEDS.